

**Interviews
with
Prof. S. Chandrasekhar**



Compiled by Symmetry Seeker

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Pure Mathematical Physics

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Interview Session - 1

Weart:

First of all, we're quite interested in how people become scientists in the first place. That's the logical place to start. I know you were born at Lahore, India in 1910, and that nearly 20 years later you came out of Presidency College in Madras, already writing significant physics and astrophysics papers. I'm very curious as to how that came about. In the first place, I don't know anything about your family — who were your parents, what did they do?

Chandrasekhar:

My father was an Accountant-General in the Indian Audits and Accounts Service, in India — a kind of a civil service. He was in the railways. He was stationed in Lahore at the time I was born. But actually, my father and my grandfather and the entire family came from southern India. Sometimes I'm considered (from Pakistan), having been born in Lahore, which is now a part of Pakistan. But actually my family and I belong to the south. In 1918 we returned to Madras, and I stayed in Madras for my

high school and college education. In 1930 July, I left India to go to Cambridge.

Weart:

Did you have brothers or sisters?

Chandrasekhar:

Yes. I come from a very large family. In fact, I am third in a family of ten. The two elder members of the family were sisters. We are four brothers and six sisters.

Weart:

So you were the oldest son.

Chandrasekhar:

Yes! The oldest son. My other brothers are quite distinguished in their own ways. My brother next to me was the general manager of the Tata Steel Works till recently. He just retired. Another brother is a distinguished doctor. In fact, he was the private physician of Shastri, when he was Prime Minister. My last brother is a radio astronomer working in the space program, in the south of India now. One of my sisters is quite a well-known musician in Madras,

and continues to be. And so are the others in various other ways.

Weart:

I see. What sort of education did your parents have?

Chandrasekhar:

My father was graduated from the University of Madras.

Weart:

Presidency College?

Chandrasekhar:

Yes, Presidency College, in fact. And he passed the Indian Civil Service examination and got into the Indian Civil Service. It was an English Civil Service at that time.

Weart:

Did you read a lot in your childhood, particularly science books? Were there any that influenced you? Were you taught at home partly?

Chandrasekhar:

We (my brothers and sisters) were taught at home, partly, but from mid-high school, what would you call probably sophomore, I went to school. I didn't go to kindergarten or things like that. I mean, I studied all at home. My father and private tutors and so on. But I started going to high school in 1922, when I was 12 years old. I went to the University in '25. I was at the University for five years. But my interest in science was something that was not unnatural, because my grandfather was a professor of mathematics, and his books were all at home. I started using them. In fact, I still have one or two books from his library.

Weart:

This is your father's father?

Chandrasekhar:

My father's father was a professor of mathematics in what is now Andhra Pradesh, in Vizagpatam (now known as Visakhapatnam).

Weart:

What about your mother? Was she educated?

Chandrasekhar:

She was educated, but not beyond high school; though she did learn sufficient English later on. In fact, she translated some of Ibsen's plays into Tamil. One of the books she translated, which had quite a wide sale, was her translation Of Ibsen's DOLL'S HOUSE.

Weart:

Oh, how interesting.

Chandrasekhar:

Ibsen has always been one of my favorite writers since that time.

Weart:

Is that where your interest in literature comes from — from your mother's side?

Chandrasekhar:

Well, I suppose it could be said that way, but my own pursuit of literature — it's rather difficult to say. I was interested in Ibsen, and later when I came to Cambridge, I got very much interested in Russian writers, Chekhov, Doestoevski, Tolstoy; and later in Hardy, Virginia Woolf, and others.

Weart:

I see. What sort of feeling did people have in your home towards science? Your grandfather had been a professor of mathematics.

Chandrasekhar:

Well, there was always an atmosphere of science. You know, my father's brother is the famous Indian

physicist (Chandrasekhara Vankat) Raman, who got the Nobel Prize.

Weart:

Oh, I didn't know that.

Chandrasekhar:

Yes. So, I mean, Raman's discovery of the Raman effect when I was still a student in India made a big impact on me, just as on anybody else. So the atmosphere of science was always at home. But actually, I would say that my really serious interest in the kind of things I did later on originated when I was in College, in Presidency College in the late twenties. Sommerfeld visited India in 1928, and I went and talked to him quite a bit. He gave me the copy of his papers on the electron theory of metals, which were then in press,* and his papers were clear enough for me to understand the Fermi statistics. At about the same time, I read Eddington's INTERNAL CONSTITUTION OF THE STARS. It's quite readable. And it was the simultaneous knowledge of

Eddington's INTERNAL CONSTITUTION OF THE STARS, together with modern statistics, at least modern as of then, through Sommerfeld, that turned my interest into the theory of white dwarfs and related matters.

*(*Zeitschrift für Physik 47 (1928))*

Weart:

I see. How did it happen that you spoke to Sommerfeld?

Chandrasekhar:

He came to Madras in 1928. I saw in the newspapers that he was going to be there, and so I went and saw him in the hotel.

Weart:

Oh, is that so? What gave you the boldness to do that?

Chandrasekhar:

Well, thinking back, it was terribly bold of an undergraduate student to go and talk to the great man. But I had read his **ATOMIC STRUCTURE AND SPECTRAL LINES** by myself, and had thought that was the end of physics. So when I went to Sommerfeld, I told him proudly that I had read his **ATOMIC STRUCTURE AND SPECTRAL LINES**, and he promptly told me that physics had changed considerably. He told me about wave mechanics.

Weart:

That was the first you heard of the new wave mechanics?

Chandrasekhar:

That's right.

Weart:

Oh, that must have been very exciting.

Chandrasekhar:

Just about the time it was going on.

Weart:

Did Raman play a role in any of this? You must have known him fairly well.

Chandrasekhar:

Oh, I knew him moderately well, but not really as well as one would think. His role essentially was to bring my attention to science. Of course, you see, the general sentiment in India at that time was quite curious. I mean, of course, it was the time when Nehru and Gandhi and others were active in politics; and like all young men, I was also very involved in that. And it was also a time when India was very proud of its men. For example, I knew about

(Srinivasa) Ramanujan and his life, and that he became the first Indian to become a Fellow of the Royal Society (in 1918).

It was all very much in the air, and of course, we were - i.e., all young students - all very proud of men like Nehru and Gandhi. It was a part of the patriotism of those times to try and see what Indians could accomplish with respect to the external world. Accomplishment in science was one way of expressing what Indians could do, you see. And I would say that this motive was present. Patriotism is a word which is not a very popular one to use these days; but Patriotism, as it was understood in India in the twenties, was one in which it was a part of everyone's wish to show that Indians could be accomplished, in a way which the outside world can recognize. To accomplish in science, to show what one could do in science, was a part of my feeling. And certainly that was one of the early motives that I had. But of course, motives in science change as you grow older. I mean, that attitude towards science is not present in me at the present time, but it was present in those days.

Weart:

This brings up some other questions that I wanted to ask you. You mentioned that you moved to Madras in 1918. I wondered—of course you were quite young at the time—how you were affected by the events at the time — the Influenza epidemic, the Famine, the Home Rule movement was going on, I suppose, still in Madras at the time, and there were the uprisings — did all of these things affect you?

Chandrasekhar:

It all affected me, in a sense. My father was very very ill, during the influenza epidemic in the twenties, and we almost thought he would die; but fortunately he didn't. But the national movement was one in which every educated person was a part.

Weart:

Did you go to a nationalist school?

Chandrasekhar:

No, the Presidency College was actually a government school.

Weart:

And when you went to high school?

Chandrasekhar:

I had four years in high school. The school was called the Hindu High School, which was an Indian high school in the sense that we learned Indian history, we learned Indian languages, we learned Sanskrit. So we were a part of that class of intellectual elite which was growing up at that time.

Weart:

Did you join Congress?

Chandrasekhar:

I didn't officially join Congress, but I remember very well when Nehru came to Madras in 1928, as the president of the Indian National Congress. He was in his thirties, and the first time he became known in the Indian movement. I went and attended it, just like anybody else. And there was the Simon Commission sent by England to visit the colleges; when the Simon Commission came, all the students went on strike, and I along with all the others went on strike and didn't go to school during those days. So the Indian movement was something which absorbed everybody of my particular upbringing.

Weart:

Was this upbringing largely secular, or was there also a strong Hindu religious component?

Chandrasekhar:

No, it was secular.

Weart:

Your parents were deliberately secular?

Chandrasekhar:

No, one can't say that they were secular; but on the other hand, in Hindu society, religious instruction is not a part of one's upbringing. It's a way of life; it naturally grows on you as time goes along. It isn't something which you get baptized in; attending churches is not a regular custom. Religious instruction per se is not undertaken as such. At least, it was not undertaken in my time.

Weart:

Your parents didn't have a particular attitude towards it, different from the other people?

Chandrasekhar:

No, it was not very different. But there is a very wide spectrum in Indian observances. I never had any religious instruction myself.

Weart:

It was simply in the atmosphere, so to speak.

Chandrasekhar:

That's right.

Weart:

I see. Now, I gather from what you said that from a fairly early age, you expected to go to college, and I suppose you expected to go to Presidency, would that be correct?

Chandrasekhar:

Not expected. We did it. It was natural that that we should have expected to follow those lines.

Weart:

Or was it a struggle?

Chandrasekhar:

Not for me, because — well, my father was in financially satisfactory circumstances. We were not particularly affluent. We had a moderately comfortable living, but we had adequate means by Indian standards; but I won't say affluent in any way.

Weart:

But in terms of your expectations you were in a class where you expected to become an educated person?

Chandrasekhar:

Oh yes. That was standard. I mean, the south Indian culture at that time was one in which to become educated, to become a graduate, to go into various work requiring an intellectual background was the normal thing.

Weart:

I see. Tell me a little about Presidency College at that time. It had just been recognized in 1923. They were trying to make the central universities stronger. I wonder, was there still and over-emphasis on examinations?

Chandrasekhar:

Oh yes.

Weart:

What was the nature of the instruction?

Chandrasekhar:

It was largely an examination oriented. But the Presidency College in Madras was the college to which all the bright students wanted to go, because the instruction was very good — in fact, in the 1900's I mean up to 1910 or 1920, most of the professors were British. I say that not in any superior sense; but it is true that these English men who taught had higher standards, and they did encourage the students to learn, not merely to memorize. In fact the students who came out of Presidency College in the 1910's and getting probably into the twenties, i.e. my time, provided much of the educated, well-known people of India. I mean, many of the students went up into the civil service. Many went into education. Many went into National movement. Many went into the legal profession. And I think, if one looks at the history of India during the fifties and sixties, you will find that many of well-known names one finds from the South came from the Presidency College.

Weart:

It was the chief university, one of the three chief, suppose, in —

Chandrasekhar:

— in India at that time. Yes.

Weart:

Were there any of your fellow students that were particularly noted, or perhaps that you kept up contact with later as colleagues?

Chandrasekhar:

Some of them went into the Indian Civil Service.

Weart:

Were there any particular fellow-students that were important to you?

Chandrasekhar:

Not especially. I left India too soon. I left when I was not quite 20.

Weart:

Were there any teachers, either in secondary school, for that matter, or at Presidency, that made a particularly strong impression on you?

Chandrasekhar:

Well, actually, I won't say that any of them made a strong impression; but I would say the following: At Presidency College, physics was taught by H. Parameswaran, who was a PhD of Cambridge and a D.Sc. of London, and quite a competent experimental physicist. His interests were different (from mine) but — understood what research was.

Weart:

What kind of teaching did you have, if you can reconstruct it? Did you have any laboratory instruction, or was it problem solving?

Chandrasekhar:

Largely problem solving. I am afraid that the education which one could obtain in those days was, from any point of view, not at all satisfactory for the understanding science as science. But it did provide a basis on which, if one had sufficient interest, one could build. It didn't make you more ignorant because the examinations were the stuff of things.

Weart:

Was it sort of like the Cambridge system in that sense, very complicated problems and so forth?

Chandrasekhar:

They weren't as complicated, but they tried to imitate it, yes; and a bad imitation.

Weart:

It was modeled on that.

Chandrasekhar:

It was their version of the English examining system.

Weart:

I see. Let me ask, did you decide at that point already to go into physics or astronomy?

Chandrasekhar:

I don't think I ever felt I wanted to go into astronomy, as astronomy, when I was young. To go

into mathematics or physics was my intention. Indeed, when I started I thought I would become a physicist.

Weart:

I see. At what point did that decision — ?

Chandrasekhar:

Well, it gradually changed because my first work was related to astrophysics, and so when I went to England, I got to know people like Milne and R.H. Fowler. And largely left to myself, as I was in England in the early years, I had to shift to problems in which I as able to be productive. And to go into astrophysics seemed to be natural.

Weart:

You mentioned that already at Presidency, you'd read Eddington's INTERNAL CONSTITUTION OF THE STARS, and I noticed in something Struve

wrote,* he mentioned that you had gotten this as an essay prize.

(*O. Struve, "Award of the Bruce Gold Medal to S.C.," *Publ. Astron. Soc. Pacific* 64 (1952), 55)

Chandrasekhar:

That's right.

Weart:

How did this happen, that they should give you this as an essay prize?

Chandrasekhar:

Well, actually, there was a prize competition to be written on quantum theory; and I could easily write on quantum theory, because I had read Sommerfeld's book; also Compton's X-RAYS AND ELECTRONS was also a book which I studied with great enthusiasm. And when I got the prize I was asked whether I wanted any particular book. And I said,

"Yes I would like to get Eddington's INTERNAL CONSTITUTION OF THE STARS," because I had seen it in the library. Of course, it was written in a marvelous language, and the early chapters are very easy to read, even for someone whose knowledge was as inadequate as mine was. It was a book I could start reading and go through.

Weart:

Did you have a particular interest in the stars, or was it simply that this seemed like an interesting —?

Chandrasekhar:

It was a book which I could read and understand. You see, after all, I wasn't taught quantum theory in school. I learned it from Sommerfeld's book, and Sommerfeld's book is one from which one could read and learn oneself. On the other hand, there were other books which if I started on my own I couldn't read and directly learn. So I started learning from books which I could understand. Eddington's was one of the books which I could understand.

Weart:

I see. I wonder, while you were still at Presidency or perhaps even before, when you began to see yourself as being a mathematician or a physicist, did you have any picture of what sort of a life you expected to lead, or was it simply the idea of making an achievement?

Chandrasekhar:

[S. Chandrasekhar on his hopes for becoming a scientist.](#)

Well, the principal motive which urges a young man to do science is first of all, to accomplish results which will be recognized; up to a point, I suppose, one hopes that one's work will become well known. And these motives were present.

And when I went to England, I had a shattering experience; to suddenly find myself in an environment where there were people like Dirac and Eddington and Rutherford and Hardy, not to

mention all the other well known names, is a very very strong sobering experience. I was extremely optimistic in India, before I left India; but once I came to England I became very sobered if not humiliated. I didn't really know whether there was any possibility for me to accomplish in the world I found myself.

Weart:

Because you were in the presence of these tremendous figures?

Chandrasekhar:

Yes. I felt that I had to simply try to work hard and try to learn and try to do the best I can, and my future prospect of becoming well known or not was irrelevant to what I had to do day by day.

Weart:

I understand. Tell me, when you left for England or even before, what was the attitude of your family towards this choice of career?

Chandrasekhar:

Oh, they were all very much behind.

Weart:

They supported your —

Chandrasekhar:

Oh, yes.

Weart:

I see. Let's see, now. Even before you left Presidency, you had already published. You published two papers on Fermi-Dirac statistics.*

(**Proc. Roy. Soc.* 125 (1929), 231-37; *Phil. Mag.*, IX (1930), 292-99, 621-24.)

Chandrasekhar:

Yes.

Weart:

Was there anybody at Presidency who was able to check these things with you, or did you simply send them in on your own?

Chandrasekhar:

No, I sent them all to R.H. Fowler. That was how I got to know him. He communicated one of my papers (to the Royal Society), which was an encouragement.

Weart:

I see. And how did you pick R. H. Fowler?

Chandrasekhar:

Because his monumental book STATISTICAL MECHANICS had just come out. And as I told you, when I met Sommerfeld, he gave me his papers on the electron theory of matter, which continued the Fermi statistics. And glancing through the monthly notices (of the R.A.S.) I found Fowler's paper on dense matter, in which Fermi statistics was used. So it seemed to me that there was an area in which one could go right in. I could understand the Fermi statistics; I knew the theory of polytropes; I had read Fowler's paper; I could understand it. Right there, there was something which I could do. So that is that is how I started, you see.

Weart:

I see. Reading publications certainly played a much stronger role in your beginning than in most people's.

Chandrasekhar:

Yes.

Weart:

Beginning essentially from the library.

Chandrasekhar:

And to a very large extent the fact that there were available to me at that time books which someone like me could read and understand by himself — Sommerfeld's ATOMIC STRUCTURE AND SPECTRAL LINES is not known now, but if one goes back —

Weart:

It's well-known to historians.

Chandrasekhar:

— if one goes back and reads that English edition, it's a marvelous book, which anyone with an interest in science could read, and verify every single step, and understand it. So is Compton's X-RAYS AND ELECTRONS — in fact, I still have the original copy.

Weart:

Your original copy?

Chandrasekhar:

The original book which I read in those days. still have them, you see. (Bringing it from the bookshelf.)

Weart:

I see. It's in very good condition, too. My goodness, for a book of that date, it's in one of the best conditions I've seen. You took very good care of your books.

Chandrasekhar:

Yes. I was very fond of that book, and you see, it was available to me. And that book also (Eddington) both extremely well-written books.

Weart:

I see. Interesting. (Reading inscription in Sommerfeld)

Chandrasekhar:

— that is a gift to my girl friend, who later became my wife.

Weart:

Oh, she was D. Lalithambika. And you already knew her back then?

Chandrasekhar:

Yes. She was in college at the same time. She was one year junior to me in college.

Weart:

Ah, I see, so you'd already met her then.

Chandrasekhar:

Yes. And we later married in '36, when I returned to India after 6 years in Cambridge.

Weart:

Yes. I was going to ask you about it when we got to 1936.

Chandrasekhar:

This book also had a great influence on me —

Weart:

The Compton book? (Looking at the books).

Chandrasekhar:

Yes.

Weart:

In fact, during your last couple of years at Presidency you must have been working very hard at mastering these things, and reading the MONTHLY NOTICES and so forth.

Chandrasekhar:

Yes, I worked very hard at it.

Weart:

Were there any other students there who shared these interests?

Chandrasekhar:

No.

Weart:

So already you had a feeling of working in isolation. suppose it may have had something to do with your originality.

Chandrasekhar:

Well, I would say that when I was young I had a driving ambition to accomplish in science. But my whole attitude changed drastically when I came to England. I mean, not that I lost any of the ambition or the interest to work, but rather that I felt that hard work is needed, something which one has to do, and

that fanciful imagination does not help very much in accomplishing in science.

Weart:

I see. You had to discipline it.

Chandrasekhar:

Yes.

Weart:

I see. About your first impressions of England — we'll get back later to your work on white dwarfs and so forth, but in the first place, your choice to go to Cambridge, was that inevitable? Was anything else considered?

Chandrasekhar:

No.

Weart:

And you already had corresponded with Fowler?

Chandrasekhar:

Yes.

Weart:

And how was this supported? This was supported by your family?

Chandrasekhar:

No, actually I was able to get a Government of India Scholarship to go to England for three years, on the strength of what I had published.

Weart:

I see. When you arrived in Cambridge and went around to see people and so forth, you were impressed by the character of these people, their scientific character. I wonder what else you saw in Cambridge? You came in and had a completely fresh view of Cambridge and the physicists there and so on.

Chandrasekhar:

Well, I remember meeting R. H. Fowler for the first time. I met him in his rooms in Trinity. He had asked me to come and see him. That must have been September, 1930. I went up to see him, and in fact gave him the manuscript of my paper on the white dwarf limit (which I had worked on aboard the streamer coming to England) and one other paper, where I had applied the polytropic theory of that Theory of White Dwarfs. I gave these two papers to him, and he talked for a while. Of course, I was enormously impressed by these men, and sobered.

Of course, another person I got to know very well during that year was E. A. Milne. In fact, he came to Cambridge. I have there a little piece I wrote for his grand-daughter, (Miranda Weston Smith) which you might take, that contains references to Milne.

Weart:

Oh, very good, I see, "Edward Arthur Milne, Recollections and Reflections."* Very good, that will help. Maybe I should ask you before we get into that about your voyage to England and so forth — this long sea voyage where in fact you got the limiting mass for white dwarfs. I'm curious where you got the idea of bringing relativity into this problem?

*(*Deposited in Niels Bohr Library/BBA-Milne)*

Chandrasekhar:

Well, that was largely because, you know, had read Lorentz's transformation in Compton's X-Rays and electrons and I knew that the velocities (of electrons) are important. And it wasn't very difficult to see it with increasing mass the velocities were

approaching the speed of light. I extended Fowler's work first, for the $3/2$ polytrope, and I knew what the central density was, and therefore I knew the velocities at the top of the Fermi level were getting close to the velocity of light when the Mass of the Star was increasing.

Weart:

You just noticed that?

Chandrasekhar:

Yes. And so I said, "What would be the equation of state?" I could easily write down the limiting formula of the equation state. The little formula which I published in the ApJ later* was the one which I derived on board the steamer. And of course, I had Eddington's book on polytropes, so it was very easy to compute what the limiting mass was.

(* *Ap.J.* 74 (1931), 81-82.)

Weart:

I see, you simply followed the model, so to speak.

Chandrasekhar:

Yes. So there was very little else — I mean, it is something which is so simple and elementary that anyone could do it, you see.

Weart:

But you had to be able to bring together the different pieces from the different books you had read.

Chandrasekhar:

But it just happened, those were the only two or three books which I had read! And it was right there.

Weart:

Well, it's probably not a bad way to do astrophysics, as a matter of fact, at least in those days. You mentioned in one of your addresses* that you hadn't understood it at that time, but by October you understood that once you go past this limit, then you go to r equals zero. But you didn't understand the implications of that?

(* *"The Richtmyer Memorial Lecture — Some Historical Notes," AM.J. Phys. 37 (1969), 577*)

Chandrasekhar:

Not right away. But I did very soon after that.

Weart:

While you were on your voyage to England, you came up with the limiting mass.

Chandrasekhar:

That's right.

Weart:

— but it didn't particularly —

Chandrasekhar:

I didn't understand what this limit meant, and I didn't know how it would end, and how it related to the $3/2$ low-mass polytropes. But all that I did when I was in England; and wrote my second paper on it.*

(**MNRAS* 91 (1931), 456-66.)

Weart:

I see. Did you already discuss it with Fowler on your first meeting with him?

Chandrasekhar:

Well, I did. But it is very curious that neither Fowler nor Milne thought of the result as very important.

Weart:

At what point did you begin to see it as important?
When did you realize you had something?

Chandrasekhar:

I would say that I fully understood its implications by the end of 1930.

Weart:

But when you arrived in Cambridge, you still didn't know whether it was anything significant or not.

Chandrasekhar:

I knew it must be significant, because Milne was working on the $3/2$ polytropes at that time. He thought that every star must have a white dwarf core. And I couldn't see how that could be true. Because the maximum mass must be — I thought it was $9/10$ (solar-mass) because I was working with 2.5 molecular weight, so I couldn't see how Milne could be right. But I couldn't fully resolve myself. The real implications I knew, that massive stars could not become white dwarfs. But how was it going to be connected with gaseous stars? It was something which I gradually resolved to myself. I think in a paper published in 1932, in the ZEITSCHRIFT FUR ASTROPHYSIK,* I was completely clear as to what the situation was at time.

(*ZS.F.AP. 5 (1932), 321-27.)

Weart:

Well, even in the MONTHLY NOTICES in March, 1931, in the beginning of 1931 —

Chandrasekhar:

— that's right —

Weart:

You come out with the radius going to zero, the density going to infinity — what you said was that you just don't know what the next equation of state would be, that this is physically inconceivable, and therefore something else has to happen.

Chandrasekhar:

Yes.

Weart:

Did you feel that this was leading towards a new physics, a new contribution to physics?

Chandrasekhar:

The idea occurred to me several times. But I kept away from it. Because somehow, the fact that this was going to play a very fundamental role — I was not willing to draw that conclusion. I was, in a sense, too diffident to draw such a conclusion, even though the thought insistently occurred to me.

Weart:

Yes. Well, after all, you were in Cambridge. This was 1931. The positron had just come out. Things were moving very rapidly (these happened in 1933) in physics. You couldn't have avoided a feeling, perhaps, that things were ready for changes, in physics?

Chandrasekhar:

Well, it could have. But the fact is that I was really on the sidelines, you know. Very few people were interested in what I was doing.

Weart:

I see.

Chandrasekhar:

In fact, Fowler did not have an office those days. He used to meet his students in the library in the old Cavendish. And I used to stand outside the library, sometimes for an hour or two, hoping I could chance to see Fowler; and most often I did not. Somehow or other, I felt I didn't belong there. It seemed to me that there were far too many big people, far too many people doing important things, and what I was doing was insignificant in comparison. I suppose I was afraid. It's rather difficult to put myself back in those days; though I do recollect well. I know exactly how I felt, standing there, even now. But to unravel it in precise terms is not easy.

Weart:

Did you discuss your work with other students? Did you have relations with the English students?

Chandrasekhar:

No.

Weart:

Were there other Indian students perhaps that you knew?

Chandrasekhar:

No.

Weart:

So you had rooms and worked by yourself.

Chandrasekhar:

Oh yes.

Weart:

I see. What about — there were clubs that met, Kapitza club, Journal clubs and so on.

Chandrasekhar:

All that I became part of only later on after I became a Fellow of Trinity, I became a fellow of Trinity in 1933, and my life changed at that time, because I was then a part of 'Cambridge': I could sit at the same table with all these others. Gradually, I could find people with whom I could talk, discuss, and indeed become friends.

Weart:

I see. It was the first two or three years there, then, that you were in isolation.

There's a couple of other questions about these first years. You submitted this paper to the *ASTROPHYSICAL JOURNAL*, and Struve remarks

that your first paper was rejected, until you produced the detailed proof.

Chandrasekhar:

Yes.

Weart:

To Struve, I suppose?

Chandrasekhar:

No — To Edwin Frost who was then, the Managing Editor. (Pause while looking through files.)

Weart:

Ah, here's the letter.

Chandrasekhar:

Here's the letter I wrote, you see, enclosing it with a short note, and the reason I sent it was because Fowler and others were not doing anything about the paper I gave them in September.

Weart:

Ah, you just gave it to them and they said "Very interesting."

Chandrasekhar:

And they. left it at that. And then —

Weart:

How do you happen to have your own letter?

Chandrasekhar:

That is because I was editor of the JOURNAL.

(later)

Weart:

I see, you retrieved it later on.

Chandrasekhar:

Yes. And this is from Frost to Gale an Associate Editor. "Will you please decide whether we should print in the JOURNAL the enclosed article? You and some of your colleagues are doubtless acquainted with the subject," you see —

Weart:

And then, Frost replied to me after he had received an adverse report from the referee.

Chandrasekhar:

"I beg to thank you The subject is an interesting one. The paper is referred to competent critics, and I have been advised that it would not be desirable to print it in the *ASTROPHYSICAL JOURNAL*. You might like to know the critics find that the fundamental idea is sound, but he has obviously used the ordinary formula (for pressure). This formula is not valid in relativistic statistics. And then I wrote back a letter, of which I have a copy — usually I don't keep copies, but this one I happen to have, and this is the proof which Carl Eckart (who was the referee) had written.

Weart:

Carl Eckart?

Chandrasekhar:

Yes.

Weart:

Carl Eckart was the referee of your paper.

Chandrasekhar:

Yes.

Weart:

I see, and he sent you — ?

Chandrasekhar:

He didn't send me. But I have a copy of it here. In fact, this one, I picked up from the kJ files. He quotes it here, you see, Frost quotes it. And then —

Weart:

I see, he quotes it, and then you found Eckart's in the file.

Chandrasekhar:

I wrote a letter to Frost: "The referee's comments which you are kind enough to enclose objects to the paper on the grounds... I am sorry, however, that the value for the pressure I give can be derived from first principles, and does not involve any assumptions except that the relativistic effects predominate. I am enclosing separately a proof. Even if my proof satisfies the referee, I am not sure that the paper will even then be acceptable for publication, and I would like to assure the referee that my formula is correct. If however the matter is intrinsically not worth publishing, I should not press at all. In any case, as it appeared from your letter that the main cause for the rejection of the paper was a supposed invalidity which unfortunately does not seem to be justified, I thought it as well to bring that to your notice..." and so on.

Weart:

Uh huh, and there was the proof.

Chandrasekhar:

And I gave the proof. And then, you see —

Weart:

— This had gone on to Eckart, I suppose —

Chandrasekhar:

And then Eckart wrote back a letter — "I am returning Mr. C's paper and think it will be suitable for publication. I have looked through his proof of his equation 2, and it convinces me that the equation is correct. I am sorry that I was in error in criticizing his equation, but it seems to me a rather remarkable thing that this equation is true. I should not have expected it at the first glance."

Weart:

I see. Very interesting.

Chandrasekhar:

And so, they published it, you see.

Weart:

And you're going to put these with the rest of your papers in the (University of Chicago Library) Archives eventually, I trust?

Chandrasekhar:

Probably, yes.

Weart:

That's a very interesting exchange.

Chandrasekhar:

Yes. As you see, I was very difficult. I said, "maybe it is not worth publishing, if it is not worth publishing, it is all right — but don't say it is wrong."

Weart:

Why did you happen to submit this to ApJ?

Chandrasekhar:

As I told you, I had written this paper in July; and I gave it to Fowler in September and he never did anything with it, whereas he sent my other paper to the PHILOSOPHICAL MAGAZINE.* And fundamentally it is because neither Milne nor Fowler wanted to accept the fact that there was a maximum mass.

(* *PHIL. G.* 9 (1931), 592-96.)

Weart:

Because Milne wanted to use the white dwarf core within his model, is that it?

Chandrasekhar:

Yes, right. Of course, all the controversy which I had with him later in '34 was a manifestation of that —

Weart:

— right, we'll have to get back to that, I want to ask you about that. But maybe first, we get to the point where you are a Fellow of Trinity and you become more integrated into the way things went. Oh, first of all, in '33 you went to the Bohr Institute, I believe?

Chandrasekhar:

Yes: 1932 September to May 1933.

Weart:

Oh, it was in '32. How did that come about?

Chandrasekhar:

Well, largely because I'd been two years in Cambridge, and done all this work, and hadn't made any impression, to the extent that I could judge myself, on the environment. I mean, Fowler was there, and I saw him once in six months, and I was just by myself, and I did not know whether I was making any headway or not. I used to know Dirac moderately well, so I asked Dirac what I should do as I was getting rather discouraged.

He suggested, "Why don't you go to Copenhagen?" Because that was the time everybody was going to Copenhagen, you know. So I went to Copenhagen, and while scientifically it wasn't a particularly a great opportunity for me, because very few people were interested in the kind of work I was doing - I did make some very good friends. Viki Weisskopf was there, so was Leon Rosenfeld, George Placzek,

and all the people with whom I maintained friendships to the present at least until they died. (Both Rosenfeld and Placzek are now dead). Max Delbrück was also there; and we all lived in the same pension. So I had, personally, a very happy life in Copenhagen.

Weart:

What was it about Copenhagen that made it so different from Cambridge?

Chandrasekhar:

Well, largely because I had friends with whom I could talk.

Weart:

But I mean — how would it be that you could make friends in Copenhagen and not in Cambridge? There must have been something very different about the atmosphere there.

Chandrasekhar:

Well, largely because I was staying in a pension in Copenhagen; and in the pension, these other people were also living. Not like Cambridge, where you stay in a room of your own, and the only place where you can meet people is when you go to lectures, or colloquia. I didn't mix with people very well. As I told you, I felt shattered in their presence, and essentially sort of recoiled within myself. That's the way I look at it at the present time.

Weart:

And also, the other students who were there —

Chandrasekhar:

They were all doing quantum theory, you know, and other glamorous things.

Weart:

They seemed very far advanced.

Chandrasekhar:

Yes, and — whereas when I went to Copenhagen, people like Weisskopf and Placzek —

Weart:

Would it have made any difference that these people were Germans rather than English?

Chandrasekhar:

No.

Weart:

I wondered whether Bohr's personality or the arrangements at the Institute might have —

Chandrasekhar:

No, for example, I didn't belong to the scientific community any more in Copenhagen than in Cambridge. For example, I remember that every Friday in Bohr's house there used to be a tea, and I used to partake of the tea; and after the tea Rosenfeld, Weisskopf and others would go with Bohr to his study to work. I used to stay behind and play with the boys. In fact, I used to play with Aage Bohr when he was still a young boy. When I wrote to him when he got the Nobel Prize and he wrote back a very nice letter recalling the time when he used to play with me, as a tiny boy.

But the personal atmosphere was better. And I made so many lasting friendships. I'm rather pleased that in Weisskopf's recent book, he refers to the fact that I was there in Copenhagen, because most often I was so outside the main stream of things, I was never a part of the scene.

Weart:

In a way I'm surprised, considering what was going on in Copenhagen at the time. They were just finishing up the quantum mechanics, moving on to nuclear physics and so on, and in a way, I'm surprised that you weren't drawn into these fields. Did you ever feel any attraction for leaving the astrophysics that you were getting into?

Chandrasekhar:

Well, that is what one would expect, and that is what I wanted, but I came to England without really very much prearranged preparation, without any real training in mathematics. Everything I had learned, I had learned by myself. During my years in Cambridge, of course, I went to a great number of lectures, and improved my general background, learned analysis and complex functions, all such matters. But I really had two choices — either to be venturesome, and go into some new area like nuclear physics or, continue with the kind of things in which I had, by myself, done some original work. And since I did not know that I was going to be in Europe

for more than three years, I had to consider the possibility of returning to India in '34 at the latest. I could not have afforded to return to India without some record of scientific work. And the question — was I to do the kinds of things which I had learned to do on my own, in which to some extent I was able to see I was making progress, go into something more glamorous and fail totally.

For example, the work which I did on my own did provide me a fellowship in Trinity, whereas if I'd gone into some of these other things, I would have learned something new, but I wouldn't have been able to get a fellowship in Trinity. So it was a question of trying to look into the future for myself, as to what kind of a future I could have.

In 1933, after I'd finished my work for the Ph.D., I went to ask Fowler whether I had any further scope in England. He said, "Well, you can apply for a fellowship in Trinity, but I don't think you have much of a chance." I applied anyway; but I was so sure that I would not get the fellowship in Trinity that I had arranged to leave Cambridge on the day

the fellowship was to be announced. And I was going to spend one term at Oxford with Milne before returning to India. And on the way to the station, I stopped at the college to find out who were the people that had got elected. I was astonished to find my name among the people who were elected.

I remember well telling myself: "Well, this has changed my life." And in fact, it did, because if I hadn't gotten the fellowship in Trinity, I would have returned to India by the end of '33, and I do not know what my future would have been, except to say that it would have been very very different.

Weart:

That raises so many questions. I guess one that particularly comes to me is the question of whether you chose something like theoretical astrophysics because if you did have to go back to India, it would be something that could be practiced there, whereas something like nuclear physics would I suppose have been much more difficult?

Chandrasekhar:

I didn't think in those specific terms, but it was all a question as to what I could do myself. It seemed to me that here was an area in which I had broken in, and it was going. Even though I was not getting that kind of external recognition which I had hoped and thought I would get, I was still doing things which were useful. And it seemed to me that the alternative of not doing anything useful by being over-ambitious, and failing even in what I was able to do was not a satisfactory one.

Weart:

I see. The fellowship at Trinity — did you hear afterwards how it happened that you got that?

Chandrasekhar:

Well, actually, E.A. Milne was one of my referees. In fact, I have a very nice letter from him, saying that he as -he-had one of the referees, recommended it; he thought my work was very good, and so on. It

encouraged me an awful lot, because I hadn't known that my work would merit it, you see, so it was quite a surprise.

Weart:

Fowler was at Trinity. He must have played a role?

Chandrasekhar:

Yes. Fowler was also one of the electors and he must have been influenced. But you must remember Dirac as one of his students and he was in the center of all the exciting things that were going on in the Cavendish...

Weart:

How did you feel about not going back to India?

Chandrasekhar:

It never occurred to me that I would never go back to India, in those years.

Weart:

It was simply a matter of staying another three years

—

Chandrasekhar:

— in England, before my fellowship in Trinity expired. And then I had an offer from this country.

Weart:

There's many questions one could ask about Cambridge. It was such an important place, and there aren't many people who still remember what it was like. We have some books and so forth. But I'm curious, during your years at Trinity, what you particularly remember about the way people exchanged ideas? Seminars, journals, clubs, informal

places that people met to discuss physics and astronomy?

Chandrasekhar:

There was the Observatory Club, that Eddington used to run. Of course, I only got into the astronomical environment during the last few years in Cambridge.

Weart:

You were saying you were getting into the astronomical community there.

Chandrasekhar:

The astronomical community was a very small one in Cambridge back in those days. There was Eddington, Stratton, Redman (who was the associate director), and a few others. We used to have meetings every two weeks. Eddington was very accessible, one could talk to him. At least I found

him accessible once I got to know him. In fact I used to discuss quite a lot with Eddington. He was a very modest person, socially, but extremely strong-minded with respect to his own view scientifically. I used to know R. O. Redman quite well.

Weart:

What sort of person was Redman?

Chandrasekhar:

A very modest person, but very anxious to do the kinds of things he was doing. He was interested in galactic rotation and early type stars. He had been to Victoria before, with J. S. Plaskett.

I used to talk to them, somewhat, about my work. It was a very small community. Eddington used to tell quite often about his conversations with Rutherford, how Rutherford was very excited about all the things. In fact I remember one marvelous occasion, a few weeks after the Cockcroft — Walton experiment

had been performed, when Rutherford told Eddington, "I'm sure that what we are doing at the present has more to do with the stars than what you are doing at the observatory."

Weart:

Is that so?

Chandrasekhar:

Yes.

Weart:

And Eddington repeated this?

Chandrasekhar:

Yes. Eddington said, "I'm sure Rutherford is right."

Weart:

How did people feel about this idea? By people I guess I mean the astronomers, Stratton, Eddington, and so forth, about this idea of astrophysics as distinct from astronomy?

Chandrasekhar:

They were all very much in the front. Stratton was, and so was Eddington.

Weart:

In fact Stratton's title was professor of astrophysics, I believe. How did you get together? You mentioned that there was a meeting every two weeks. Was this a journal club or seminar?

Chandrasekhar:

Something like that. It used to be called the Observatory Club, which Eddington use to run.

There used to be teas which Eddington's sister used to serve.

Weart:

Was this in his house?

Chandrasekhar:

His house was adjoining the observatory, so it used to be just outside.

Weart:

I see.

Chandrasekhar:

We used to have tea there.

Weart:

Would the physicists come?

Chandrasekhar:

Not many. Occasionally. Hugh Newall was still alive in my time and he used to come. The same way with the Cavendish Club which Rutherford used to preside over on every occasion.

Weart:

You would go to that?

Chandrasekhar:

Yes, I used to go to that. J.J. Thomson was still around, so was Aston, Chadwick, Blackett, Oliphant, Kapitza, Dirac and a whole lot of others.

Weart:

Were there informal places that you used to get together with astronomers or physicists?

Chandrasekhar:

In Cambridge in those days the people with whom you got together informally were the people who belonged to your college. For example, I was in Trinity so I used to see a good deal of people who were in Trinity. Rutherford used to dine every Sunday, so I used to see him on Sundays. Eddington used to dine four to five times a week, so I used to see him, I used to know J.C. Littlewood as well as, Hardy, Aston, J.J. Thomson, Gowland and Hopkins (who was a biologist). All these people one met either at tea — there was a common parlor that you could go to for tea and you'd see them — or at dinner you'd talk to them.

Weart:

What sort of things were talked about? Did you, for example, talk about philosophy and politics, that sort of thing?

Chandrasekhar:

Usually one doesn't talk shop at the high table. Rutherford used to, though. He was an exception to everything. Usually after dinner one goes into the parlor for coffee and people usually sit around in groups. I remember particularly during the Christmas recess, people used to sit around the fire and talk a good deal.

Weart:

I know, as you mentioned, you were getting interested in literature at that time. Were there other outside interests, with your general education?

Chandrasekhar:

I belonged to a group of young students in my Trinity fellowship years, who were what I suppose one might call avant-garde. People who were friends of F. R. Leavis, who was still a young man in those days. (Now he's retired.) In fact the wife of one of my friends was a literature graduate of Cambridge. She was blind. She was a strongly sensitive woman and she influenced me to read Virginia Woolf. I started reading her in those days, and have consistently read everything she wrote since that time. And James Joyce.

Weart:

You must have had some interesting conversations with Jesse Greenstein.

Chandrasekhar:

Yes. I also read most of Chekov, Tolstoy and Turgenev, I read all of them in those days.

Weart:

I suppose this was very much in the Cambridge spirit to do this sort of thing.

Chandrasekhar:

Yes.

Weart:

In your own field, it's always interesting to know what books or journals in particular you read that impressed you. What journals would you read regularly? What books came out that were important?

Chandrasekhar:

I used to read the MONTHLY NOTICES [of the Royal Astronomical Society] quite regularly, and the ApJ. The books I read were largely astrophysics books. Milne's books and Milne's articles. But as far as astrophysics went, I learned most of it from

periodicals. For example, I remember starting with MONTHLY NOTICES in 1920. I tried to read all the articles on astrophysics.

Weart:

Reading straight through?

Chandrasekhar:

Yes.

Weart:

And afterwards, when you were in the middle '30s and you were a little more established, did you read it straight through every time it came out?

Chandrasekhar:

I certainly used to read all the articles on stellar structure and stellar atmospheres.

Weart:

And the ZEITSCHRIFT FUR ASTROPHYSIK?

Chandrasekhar:

Some. The ZEITSCHRIFT FUR ASTROPHYSIK was founded only in the '30s—'31 or '32 it came out. I used to look at it regularly and read the papers —

Weart:

And read the ones that were of particular interest to you. I see. Were there any other journals you would look at?

Chandrasekhar:

Well, the standard physics journals. But I wasn't very much involved in it.

Weart:

Did you read NATURE?

Chandrasekhar:

Oh yes, NATURE, quite regularly.

Weart:

PHIL.TRANS.? —

Chandrasekhar:

— The Royal Society PROCEEDINGS. I used to look at them but not read them. I use to read books in physics. I mean when books on nuclear physics came, like Rasetti's,* I used to read them.

(* *Franco Rasetti, ELEMENTS OF NUCLEAR PHYSICS (1936)*)

Weart:

Bethe's REVIEWS OF MODERN PHYSICS articles (1936-7)?

Chandrasekhar:

Yes, those articles. I used to learn physics from books. But astrophysics, I don't think I ever learned from any book. I think the only book in astrophysics I read completely was Eddington's INTERNAL CONSTITUTION. But most of it I learned as I went along.

Weart:

You would select the pieces you needed at that particular time?

Chandrasekhar:

Yes.

Weart:

Who were the people that you looked up to? You mentioned Fowler of course, and Milne, but in particular in your field, stellar structure and so forth, who were the people whose articles may have impressed you?

Chandrasekhar:

Russell, of course, came in a little later — I would say I was largely brought up on Eddington, Milne, Jeans, Russell, and among the other people, Stromgren, Biermann, and Cowling. Beside I also read Ambartsumian paper in the PULKOVA BULLETINS.

Weart:

It was published in French, wasn't it?

Chandrasekhar:

From Pulkova — it used to be published in English in those days, the PULKOVA BULLETINS.

Weart:

I see. While you were at Trinity as a fellow, did you visit other places also?

Chandrasekhar:

Some. For example, in 1931 I visited Gottingen.

Weart:

Oh, I didn't know that. How did that come about?

Chandrasekhar:

Well, you know, I always wanted to be a physicist, and so I wrote to Born and asked him if I could come and spend the three months of the summer there. So the first summer I spent in Gottingen.

I went to Russia in 1934 and saw Ambartsumian — that was how I got to learn about him. And Gerasimovich.

Weart:

What was Ambartsumian like at that time? What was Russia like at that time?

Chandrasekhar:

When I went, it was before the great Purge; Stalin's Purge of '34. It was very free when I went.

Ambartsumian was very free and very open. He was extremely critical of his seniors. Ambartsumian, Landau and I went for a walking trip outside Leningrad, we went into the Forest outside Pulkova. It was in August. It was nearly the "white nights." I visited the hermitage with them.

Weart:

What was the Russian astrophysics or astronomical community like at that time?

Chandrasekhar:

Ambartsumian and Kosirev - (later spent decades in Stalins prison camps). They were the people whom I knew best; I knew also Gerasimovich and Shajin — there was a man called Kratt. I knew of their work a little before, because they published in the ZEITSCHRIFT FUR ASTROPHYSIK. I got to know Ambartsumian and Kosirev and followed their work consistently afterwards; Ambartsumian particularly.

Weart:

That's a very important community. We knew very little about it. I wanted to interview Ambartsumian at the last IAU but he couldn't come, and I wonder if you saw them at work — did they get together in

seminars, for example? How would it compare to a place like Gottingen or Cambridge.

Chandrasekhar:

I think Ambartsumian mixed with the physicists. He was a great friend of Landau. So I rather imagine that he, Landau and the younger people formed a group by themselves. And certainly they were right there on the top at that time.

Weart:

Was Ambartsumian already the acknowledged leader?

Chandrasekhar:

I thought he was marvelous. My own impression has always been that he was, when he was in his prime, one of the most perceptive and elegant of astronomers. But it seems to me that after the middle

forties, he's been much more of a politician than an astronomer.

Weart:

But at that time, he had a magnetism?

Chandrasekhar:

He certainly was very sharp, and very precise, and his papers were beautifully written.

Weart:

Of course, I suppose you must have traveled around England and gone to various conferences and so forth?

Chandrasekhar:

Not very much. I used to go to the RAS meetings regularly.

Weart:

OK, back to Cambridge, then. We haven't talked much about the people. You said a little about Eddington and Fowler, but I still don't have too much of a picture. Eddington, for example, you say was not too social, he was a retiring person?

Chandrasekhar:

That was the general impression he gave. But once you got to know him, he was really very sociable, in the sense that I had no difficulty talking with him. We talked for a very long time. But of course, you know if I start talking about Eddington, I could talk for a whole hour about him.

Weart:

Well, maybe we shouldn't talk for an hour about him but one would like to know more about him. What was he like?

Chandrasekhar:

Let me see. He was a man who was very distinguished, in the sense that one felt when one talked to him that one was talking to someone really substantial. The British, particularly in earlier times, can be very nice and kind, but at the same time, an element in their behavior makes it very clear that they're on a different level. There's no snobbery involved in it. It sort of comes naturally to them. Eddington was a man of that kind. Scientifically, he was an extraordinarily self-contained man. I don't think he was ever inspired by other people along directions which he did not believe in. One characteristic of him which was extremely typical is manifest in one of my controversies with him.

Eddington said, "You look at it from the point of the star. I look at it from the point of view of nature."

I asked him, "Aren't they both the same?"

He said, "No."

Well, you see, that sort of shows his attitude. Somehow, he felt that nature must conform to what he thought was right.

Weart:

Almost a philosophical —

Chandrasekhar:

Yes. And he was of course very devout. In fact the Royal Society, during the time I was in England, sent a circular to all their fellows, whether they believed in a Divine Presence, and Eddington was among the very few people who replied, in the positive.

I heard his very famous lecture, "Science and the Unseen World," which is very well known, in which he made some marvelous remarks. "There's a kind of sureness that is not cocksureness" — much marvelous, beautiful statements.

Weart:

And he communicated this also in his personal relations?

Chandrasekhar:

Yes.

Weart:

. How was he regarded by the other people around the observatory or around Cambridge?

Chandrasekhar:

Oh, his position in astronomy was dominant, what Eddington said, was right. I don't think there was any doubt in anybody's mind that Eddington was always right. For example — well, I shouldn't call it famous — the meeting at which Eddington and I disagreed. I gave a paper and then —

Weart:

In '33 —

Chandrasekhar:

'34, January.

Weart:

Oh, in '34, this was an RAS —

Chandrasekhar:

— meeting, and I gave a paper. Eddington came up and said, "The paper which has just been presented is all wrong."

He then made a lot of jokes and at the end of the meeting, everybody came and said to me, "Too bad.

Too bad." The other astronomers were certain that my work was wrong because Eddington had said so.

Weart:

"Too bad" meaning, "Too bad that you had got it wrong"?

Chandrasekhar:

Yes.

Weart:

What did this do to your personal relations with Eddington?

Chandrasekhar:

It did not affect our personal relations: that is not the Cambridge Style! We remained very good friends. But even now, when I think of him outside of the context of my controversy with him, I have the

nicest feelings about him. But he was very, very obstinate. Very obstinate, right up to the end.

Weart:

Did other people at Cambridge have similar difficulties with him?

Chandrasekhar:

Nobody (except Milne beside myself) crossed any swords with him; and Milne lost grievously. So far as I know, I was the only one who crossed swords with him, and history has shown that he was wrong. But in every other case ... But I don't think Eddington ever conceded. Oh no, he was convinced of this correctness right to the end.

Weart:

People would generally be careful of him.

Chandrasekhar:

No, I mean — just consider the fact that in the early years, when people were talking about white dwarfs, and when some of these objects were not white any more, I asked then, "Why don't you call them degenerate stars," and they said, "No, because that would imply that I'm accepting your theory."

I think Eddington's influence on this matter remained right up to the end.

Weart:

Not just on the Cambridge people but outside also?

Chandrasekhar:

Take Russell. The following incident illustrative of Russell's attitude. At the IAU meeting in 1935 — Eddington was the President of the commission on the "internal constitution." Russell was the secretary and presiding. With Russell presiding, Eddington gave an hour's talk, criticizing my work extensively and making it into a joke. I sent a note to Russell,

telling that I would wish to reply. Russell sent back a note saying, "I prefer that you don't." And so I had no chance even to reply; and accept the pitiful glances of the audience.

Weart:

My goodness!

Chandrasekhar:

No, I don't think that there was any doubt in anybody's mind in those days that Eddington was right, by virtue of Eddington's extraordinary dominance.

Weart:

I suppose other people hadn't thought about it very deeply or hadn't tried to look into it.

Chandrasekhar:

Yes.

Weart:

Eddington said it was so, and it was so. I see.

Chandrasekhar:

Eddington had an absolutely dominating influence.

Weart:

I would think this would have suppressed some other developments, in that case.

Chandrasekhar:

Well, if you read my article on Milne, you will find that he effectively destroyed Milne.

Weart:

I was going to ask you about that the next. You said you had good relations with Milne quite early, but he wasn't at Cambridge?

Chandrasekhar:

No he was not at Cambridge. He was at Oxford. But my work was related to his. And so I corresponded with him. But very soon I found that Milne wanted to just develop his ideas along his own method. All these people — Eddington, Milne and others — knew perfectly well how nature was built. Their work was to confirm that their own ideas of nature were correct. I don't think they ever tried to explore nature with the intent of unraveling it as time went along - never allowing the investigations to reveal nature.

Weart:

They began with the accepted laws of physics — or, in Eddington's case, what he expected the laws of physics to be?

Chandrasekhar:

Well, in the case of Milne, he refused to accept the physics. He just said that degeneracy must be wrong because it contradicts his ideas, meaning 'common-sense.'

Weart:

Getting back to the origins of these things, I was struck when I looked again at this 1931 paper of yours, when you first came out with the fact that it would have to go to zero radius and so forth — that it immediately follows in the journal a paper where Eddington is arguing with Milne.* Eddington gives a theorem here which he says "May be of use in curbing riotous speculations which go beyond the temperature of 109 degrees and a density of 106."

Which of course was Milne's speculations. And then, you give a paper which supported Milne.

(* *MONTHLY NOTICES* 91 (1931), 440, 444.)

Chandrasekhar:

That's right. But Milne, again, for different reasons wouldn't accept mine.

Weart:

Yes, I'm curious about that. Do you suppose the fact that you began by supporting Milne may have influenced Eddington's view of your work?

Chandrasekhar:

No — you see, if you want to take this controversy, the situation as far as Eddington's part in the problem was the following: First, it ought to be said that Eddington did not read anybody's else's papers. For example, he refers to Fowler's work as having applied the Einstein-Bose statistics.

Weart:

Not the Fermi-Dirac?

Chandrasekhar:

Not the Fermi-Dirac. In his first edition of his *THE NATURE OF THE PHYSICAL WORLD*, you will find that he says Einstein-Bose. And his account of Fowler's work at the RAS used Einstein-Bose. So, you see, Eddington had an idea that every star, when it cooled, must have a finite state in which it can exist. On the $5/3$ law of degeneracy every star can become a white dwarf. So for Eddington that is the way it must be. On the other hand, if there's a maximum mass for degenerate state that is not permitted. And as Eddington stated, in his own writings — I've quoted many of these in my papers — he said that the only possibility is that "a star must go on radiating and radiating and contracting and contracting, till at last it finds its peace as it recedes into its gravitational radius." Now, that clearly shows that in 1934, Eddington realized that

the existence of a limiting mass implies that black holes must occur in nature. But he did not accept that conclusion. He said that must be a reduction ad absurdum. Eddington's enormous physical insight clearly showed that black holes must occur once one accepted physics. If he had accepted that, he would have been 40 years ahead of anybody else. In a way it is too bad.

Weart:

Yes.

Chandrasekhar:

Eddington wouldn't accept the maximum limit for his own reasons. And Milne would not accept it, because he thought that every star must have a degenerate core for different reason.

Weart:

And this was simply because they were stuck on the way that they had first approached the problem?

Chandrasekhar:

Just about that, yes. It's because they had preconceived ideas. This is effectively what Milne wrote to me.

Weart:

You have this paper that you loaned me on Milne.

Chandrasekhar:

Yes, I quote it in other places. [Looking through the paper on Milne.]

Here. Milne wrote me: "If the consequences of quantum mechanics contradict very obvious much

more immediate considerations, then something must be wrong either the principles underlying the equation -of- state derivation or with the aforesaid general principles. Kelvin's gravitational -age-of-the-sun calculation was perfectly sound; but it contradicted other considerations which had not been realized. To me it is clear that matter cannot behave as you predict." And he continued, "Your marshaling of authorities such as Bohr, Pauli, Fowler, Wilson, etc., very impressive as it is, leaves me cold."

[Reading from the paper on Milne:] "From the vantage point of today, it is clear that Milne's negative attitude prevented him from realizing that the incorporation positively, of the consequences of Fermi degeneracy leading to an upper limit to the mass of white dwarfs, leads one directly to conclude that massive stars, after they have exhausted their sources of energy, must collapse to black holes — a conclusion which Eddington drew but which neither Eddington nor Milne would accept."

Weart:

Right. I understand. I'm curious, you mention the fact that Eddington drew this conclusion — was it in conversation with you?

Chandrasekhar:

Actually, he has written that.

Weart:

Where did you see it?

Chandrasekhar:

It is published in THE OBSERVATORY*

(* *"THE OBSERVATORY" Vol. 58 (1935), 38.*)

Weart:

Right, but I'm interested — did he reach this conclusion while he was talking with you?

Chandrasekhar:

Well, the history of this is approximately as follows. I found the limit in 1930, it was published in '31. And then, I resolved the nature of the problem in my own mind in 32. But all this time, I had not worked out a complete theory of white dwarfs, in which I used the exact equations. I did that for the first time in the fall of 1934.

Weart:

Right, published in '34*

(* "*OBSERVATORY*" 57 (1934), 373-77; see *MNRAS* 95 (1935), 207-25, 226-60, 676-93.)

Chandrasekhar:

And during the time I was working on this, was a fellow of Trinity, and Eddington used to come to my rooms in Trinity, every so often, after dinner, to see how my calculations were progressing. How my

mass-radius relation was coming out. And indeed in its general progress.

Weart:

I see, he was very interested in this. Anxious.

Chandrasekhar:

He was very interested, and anxious to know. And then, I was scheduled to give an account of this paper at the January '34 meeting of the Royal Astronomical Society. The meeting is generally on the second Friday of each month. On the Thursday before, I was dining in college and I saw Eddington. Now, the assistant secretary used to send me the program for the meetings.

Weart:

The secretary used to send you the program?

Chandrasekhar:

Actually, the programs are not sent out before the meeting, but I happened to know the assistant secretary, Miss K. Williams and she sent it to me privately, because I was on the program.

Weart:

I see.

Chandrasekhar:

And when I got the program, Thursday evening, I noticed that after my paper, Eddington was to give a paper on the relativistic degeneracy. And I was really annoyed, because here was Eddington coming and talking to me, week after week, about my work while he was writing a paper himself and he never told me about it.

Weart:

Never told you about his views, just came to look at your calculations.

Chandrasekhar:

Yes. And talking to me all the time about the work. And I was telling him, "How can a star evolve? Massive stars must behave differently," and so on — all this was being talked about.

And then I went to dinner, and Eddington was there, and I was still annoyed, because he had never told me. After dinner, I didn't try to go to see him. But he came up to me. And even then he wouldn't tell me. He only said, "You know, your paper is so long that I asked W.M. Smart who has the secretary with RAS at that time to give you half an hour instead of the normal 15 minutes, so that you can explain your work properly."

And the following day, at the Burlington House where the RAS meetings use to be held, I was

standing together with W.H. McCrae. Eddington came by, and McCrae asked Eddington, "Professor Eddington, what are we to understand about relativistic degeneracy?"

Eddington turned to me and said, "That's a surprise for you." Then at the meeting, I gave my paper, and Eddington got up soon after that and said, "I do not know if I shall leave this meeting alive, because the paper which you have just heard, the foundations of it are completely wrong."

Weart:

And this was the first you have heard of those views of his?

Chandrasekhar:

That's right.

Weart:

My goodness!

Chandrasekhar:

And then he went on to make some remarks which were mainly if you read the OBSERVATORY you will find, "laughter" interposed in many places. And that was that. And at the end of it, everybody came by and said, "Too bad." "Too bad." And of course, you know, Milne on the other side did not want to accept my work because it contradicted his work in other ways. I had Milne come and visit me in Cambridge during the fall of '34, to talk to him about my work, and to get him prepared to accept my work. And he had pretty well accepted it, but when Eddington said that the formula was wrong, Milne was all aglow.

I remember going to Paddington Station before going to Liverpool St., that evening, and Milne came to me and said, "I feel in my bones that Eddington is right." I told Milne, "I wish you felt it elsewhere." I was so angry. So that was the way it sort of ended.

In many ways, thinking back over those times, I am sort of astonished that I was never completely crushed by these Stalwarts. You know, none of these

people would accept my work, astronomers wouldn't accept it and finally in 1938, I decided that there was no good my fighting all the time, that I am right and that the others were all wrong. I would write a book. I would state my views. And I would leave the subject. That's exactly what I did.

Weart:

Now, in 1938 there was an IAU symposium. I remember, the first feeling I had about any of this was some time ago, when I happened to read Eddington's report, the 1938 IAU Section report, and he puts in completely gratuitously in his report his feelings about degeneracy and so forth, and I looked at this and I said, "What is going on with Eddington?" Eddington clearly was upset about something. And this was before I even knew that there was a controversy.

Chandrasekhar:

There was a discussion after that. I don't know if you have read that. Eddington and I really talked to each other in strong language.

Weart:

No, I didn't read the discussion, I just saw Eddington's summary report.

Chandrasekhar:

Well, I think the PROCEEDINGS have been published. If you happen to look in it, I won't go through that in detail, but there's a long discussion. I remember, at the discussion, Kuiper asked Eddington, "Well, Professor Eddington, there are two theories of white dwarfs. How can an observational astronomer distinguish them?" And Eddington said, "There are no two theories." I got really angry. I got up and said, "Well, Eddington, how can you say that there are no two theories? Because we were in Cambridge just the other day, in a discussion with Dirac and Peierls and Maurice H. Price, and all three did not agree with your work on degeneracy. And to the extent that these distinguished physicists think that my formula is right, an observational astronomer must conclude

that there are two theories." At this point, Russell got up and said, "The discussion is closed." That was the last of that.

Well, that afternoon you asked me about the personal relations — that was the last part of the meeting where there was a big reception and lunch at the City Hall. All of the Paris great were there; Langluis, Joliot, de Broglie, Curie and all the others they were all at the high table, and I was way off in the corner somewhere. At the end of the meeting, I was standing by myself when quite suddenly I found Eddington next to me. He said, "I hope I did not hurt you this morning."

I asked him, "You haven't changed your mind, have you?"

Eddington said, "No."

And I asked, "What are you then sorry about?"

Eddington just looked at me and walked away. That was my last conversation with Eddington, because he died a few years later, you know, during the war.

Weart:

So the personal relations maintained a correctness.

Chandrasekhar:

Yes. And actually, after the war began I wrote him several times; in fact, I had a letter from him dated two days before he died. He said that he's going to the hospital because he's not feeling well, and so on, but "when I came back, I'd do these things." And when I got the letter, he had already died.

So we kept our personal friendships till the end. I have very warm personal feeling towards him.

Weart:

Now, about Milne's role in all this, you mentioned the problems Milne had with Eddington. In a way, I'm surprised Milne should have supported Eddington. What were Milne's feelings towards Eddington?

Chandrasekhar:

I think I explain all that pretty well in that article I gave you.

Weart:

OK.

Chandrasekhar:

But one thing is certain in my mind: Milne's enormous originality was frustrated by his attitude of trying to do science which would contradict Eddington all the time. In this one respect he went along, because in this respect his arguments in a different context makes his ideas contradicting Eddington right. It's one of those inverted things, you see.

Weart:

Would this have happened to Milne, do you suppose, if someone else had been the recognized great authority? Or was it specifically Eddington?

Chandrasekhar:

It is hard for people to realize what an incredibly dominating position Eddington had during his life. For example, Shapley told me this: in 1936, they had a tricentennial at Harvard, and, Shapley said, they sent a circular around to American astronomers, to rank astronomers so they could give honorary degrees. And he said that Eddington was the first in every single list he received! And in one of them, Eddington [at the top], 30 dots and then Jeans.
[Laughter]

I think that is most unfair, as far as Jeans was concerned, but the fact is that there was not a single astronomer in the thirties who would not with unanimity have said that Eddington is the greatest

living astronomer. He had an absolutely dominating position.

Weart:

Was it this position, do you think, that attracted Milne's attention?

Chandrasekhar:

That is one. But Eddington in science can be most aggressive and abrasive. I mean, he will do things which are quite often terribly rude.

For example, Jeans sent a paper on radiative viscosity. The program contained it. Eddington sent a postcard to Jeans, deriving the formula for radiative viscosity. Because once the concept is stated, anybody can write the formula down.

Weart:

Right. Once you see that word, "radiative viscosity."

Chandrasekhar:

But isn't it insulting that after seeing the title, you send the formula to somebody else to show "I'm as good as you." It's childish.

Weart:

It's funny, in some ways what you're saying about Eddington, particularly in the English scene, reminds me of Russell in some ways in the American scene.

Chandrasekhar:

Yes.

Weart:

I'll have to ask you later about Russell, when you came to the United States. I want to ask you also about your personal relations with Milne. After all, Milne had introduced your early papers and so forth,

you even credit him with helping you, with conversations on your first papers.

Chandrasekhar:

He remained a personal friend all through his life. Look, [pointing to wall of office], that's his picture there. The first one, on the top, is E.A. Milne. He visited us in this country in 1939, and that photograph was taken when he was visiting us.

He remained a personal friend, and I'm deeply attached to him. You know, Milne had a marvelous intellect. He was the first scientist who really encouraged me. When I was at Cambridge in 1930, the first year, I told you that I done this work on the white dwarfs and that Fowler had sent it on to Milne. Milne visited me in my rooms in Cambridge, by himself, came unannounced one afternoon to see me. I was most touched by it. For example, in my book on stellar structure, I refer to Milne at every place where I agree with him. But I never refer to any of our disagreements. Because I knew that it would

hurt him enormously, and so I preferred not to refer to our disagreements. I state my own views correctly, and whenever my work impinges on his and he's right, I mention him in those places. He noticed it, and in fact, in one of my last conversations with him, he told me that he had noticed that fact and greatly appreciated my personal courtesy.

My correspondence with him extended right up to his death. In fact, I got a letter from him aboard the steamer going to Belfast, the morning after which he died. He was a very great personal friend of mine: the first and the best.

Weart:

Who else were your friends in England? You mentioned Dirac.

Chandrasekhar:

Scientifically, McCrae and I became rather good friends. We have continued to be good friends. McVittie was another. And George Cowling. These were my contemporaries in Cambridge at that time.

And among the physicists in Cambridge, there was a low-temperature physicist, David Schoenberg. I knew the mathematicians very well, Harold Davenport, (most particularly) Hans Heilbron, Donald Coxeter, Patrice Duval, and A. Besicovitch.

Weart:

Speaking again of the physicists, you mentioned, the letter of Milne that you quoted, speaks of the physicists whose support you could marshal on degeneracy, and again at the IAU meeting, Dirac and so forth, supporting you. Did you get a real feeling of support from the physicists at Cambridge?

Chandrasekhar:

Yes. Yes.

Weart:

So when you say "it was not accepted," it was strictly the astronomers and astrophysicists.

Chandrasekhar:

R.H. Fowler accepted me. In fact, he told me, "Don't worry about Eddington." And Pauli, wrote pretty nearly the same thing.

Weart:

How did the physicists relate to the astronomers there? And the astrophysicists?

Chandrasekhar:

Well, you know, astronomy was never an integral part. In fact, I just got recently a book of scientific quotations, in which Rutherford said, "Don't talk about the universe here." And there's also another quotation of Rutherford, when someone asked him about Einstein — "Oh, that stuff, we don't bother about it." Of course, Rutherford was an extrovert, very open, and very often he would say things merely for the effect; so one couldn't really go deep into it. But I think it sort of gives you the overtones.

Weart:

What about some of the others, Dirac and people like that? How much interest did you feel that the physicists had in what you were doing, or in general, what was going on in your field?

Chandrasekhar:

Not at all. For example, during the first year in Cambridge, I used to know Dirac very well, because Fowler was away, and Dirac told me, "Well if I were you, I would be interested in relativity, rather than astrophysics."

I asked him, "At one time you did write a paper on astrophysics." Dirac said, "Oh, that was before quantum mechanics." No, I am afraid that astrophysics was considered inferior by most physicists. In fact all physicists.

Weart:

Did the astrophysicists react to this?

Chandrasekhar:

In a curious way.

I remember Wigner telling me, during the war, "Well, astronomers are very, very clever — they have thought of 1 percent of the possibilities."

That's typical. I think the rather high position which astronomy and astrophysics occupy now is a consequence of the sixties. Then a whole sequence of new discoveries was made, and the relationship with relativity became very important — gravitational collapse, black holes and so on. And the situation has changed.

Weart:

So the feeling was at the time, if one discovers something in nuclear physics, that might be applied to astrophysics, but not the other way around?

Chandrasekhar:

That's right. For example, I am sure that if you ever have a chance to talk to Bethe, Bethe could tell you if he still remembers, that the attitude of the physicists toward the astronomers was one bordering on contempt. Maybe "contempt" is a little too strong.

Weart:

Did you get this everywhere?

Chandrasekhar:

Yes, I'm afraid so.

Weart:

Not just at Cambridge, but Gottingen, Copenhagen?

Chandrasekhar:

There were exceptions. Men like Eddington had enormous personal prestige. But Eddington was the sole exception. Jeans, for example, had very little — I mean, whatever respect people had for Jeans, in England, derived from his very successful ten-year tenure as the Secretary of the Royal Society.

lifted(* *PROC. AM. PHILOSOPHICAL SOC.* 108 (1964),)

Chandrasekhar:

Ok, yes, to the extent nuclear physics was involved.

Weart:

But just as an application of nuclear physics.

Chandrasekhar:

Well, I'll tell you an interesting conversation I had with Bohr, when I was in Copenhagen 1932-33. I

was working on rotating stars at that time. Bohr said, "Well, I was interested in astrophysics when I was young," and he recalled the fact that the helium lines (the Brackett spectrum,) lifted Balmer's formula for half integral quantum number. And then he said, "more lines must be due to the ionized helium lines," and one should be able to detect the difference in the wave lengths because of the mass effect. A. Fowler had found that, you see. [Distinguished spectroscopist who was a Professor in Imperial College in London. He also led some eclipse expeditions.]

But then he said, "Well, I've always been interested in astrophysics, but the first question I should like to know about the sun is: where does the energy come from? And since I can't answer that question, I do not think a rational theory of the stellar structure is possible."

Well, great as Bohr is, that remark of Bohr's is invalid. Later on, if one found the right nuclear reactions, it was because one had found out earlier

the right temperatures and physical conditions by their ingenuity.

Weart:

Right. They didn't realize that at the time.

Chandrasekhar:

You know, it was the standard thing to say. "I look at the sun. Can you tell me how it radiates? You don't. So why should I believe anything else you say?" It looks childish — an attempt that one cannot explore a question like that.

So, I am afraid that the physicists did not think much of astrophysicists. And in a way, it is understandable. So many things were happening in physics. And astrophysics was trying to get an insight into a subject the fundamental of which one did not understand; and that is not a particularly rewarding job for a person who is finding new laws of nature at every other turn.

Weart:

This brings me very directly to the next thing I wanted to ask you, the next subject — the rotating stars you were mentioning about discussing with Bohr, is what came out in this series, starting 1933, "The Equilibrium of Distorted Polytropes." In fact, we've done some citation counts, and this seems to be one of your most celebrated papers from the 1930s. I'm curious about this, and how you came to this particular problem?

Chandrasekhar:

If you want to know the truth, there is nothing exciting at all. January, 1933 came along, and I knew that my scholarship expired that August, and I knew that I had to get my PhD, write a PhD thesis, and I looked around and asked, what it is I could do? And I saw an old paper of Milne's, 1924, on rotating stars, I wasn't too satisfied with what he had done, so I thought, well here is an area in which I could work up a whole new subject; and that is how it started.

Weart:

So to you it was simply like a tripos problem, so to speak, a mathematical problem?

Chandrasekhar:

Yes. That is how it started, yes. To call it a 'tripos problem is perhaps not quite fair!

Weart:

Did you do this under anyone in particular?

Chandrasekhar:

No, I just did it on my own.

Weart:

I'm interested, because this looks to me like one of the first of your papers which really is what's become well known as a Chandrasekhar type of

paper — it has a lot of mathematics, it takes a particular kind of problem and really works it through, and so forth. I wonder where you got this sort of feeling? This was to produce a thesis, but it seems to be part of a larger feeling that you should do this kind of thing.

Chandrasekhar:

Well, I think I tried to state the underlying motive, but on the other hand, what fascinated me about the subject was, here was an area in which one can do some definitive work.

I think that it probably was the first time that I began to build up this feeling, which I have maintained all along; namely, that in astrophysics, if one wanted to do some work which would have some degree of permanence, it ought to be well-defined, complete, rigorous, and relevant — in the long run, not necessarily in the short run. That has been my principal motivation in my work all along.

When I started this work, even though I had to write a thesis, I felt very enthusiastic about it, because I said, "Well, this work which I do now will be a permanent contribution, to the extent I can see, because it will be done and finished and that will be right, so long as people want to use it."

Weart:

There will always be rotating polytropes.

Chandrasekhar:

Yes. No matter where one goes. And history has shown that, more or less, my idea is right. Similarly when I wrote my book on the ellipsoidal equilibrium,* it was the same feeling, because I said, "Well, people may not 'think much about this subject, but it is a well-defined subject, lots of people have worked on it, it is in a terrible mess, I will straighten it all out and write a book, and leave it; it will be there, if people care about it." Actually, it has turned out that that book is now used quite extensively, and provides the models for lots of things people are doing these days.

(**ELLIPSOIDAL FIGURES OF EQUILIBRIUM*
(Yale Univ. Press, 1969).)

That has in fact been my primary motive in astronomy. In fact, I said somewhere that my attitude, which first is exemplified in this work on rotating stars, is that theoretical astronomy must provide for astronomy what experiments provide for physics. In other words, you must provide a basis of calculation which is so right that nobody can argue about that. So you can integrate it into your observations, as something that is valid.

Weart:

Given the initial assumptions.

Chandrasekhar:

And the assumptions are relevant. I mean, it may be a model, but it illustrates the problem, and it provides a basis, so that on this assumption, this conclusion is right. There's no argument about that.

Weart:

I see. This style, this program, which has been very successful, does not, however, seem to be a style or program which is too common. I wonder, why did you pick it up? Is it something that you picked up at Cambridge? Is it something in your personality? Something from Madras, even?

Chandrasekhar:

No... One moment. [Gets article.] Here is an article in NATURE* which I —

(* "*Development of General Relativity*," *Nature* 252 (1974), 15-17.)

Weart:

Ah, no, I didn't see that one.

Chandrasekhar:

You can have this. I say here, "By saying that astronomy is a natural home for the general theory of relativity, and I am suggesting a role for theory in astronomy which is not generally accepted; in my judgement theory has a double role to play in astronomy; the common one of providing interpretations for observed phenomena, and the uncommon one of providing for astronomy the kind of basis which experiments provide for physics. The latter role is largely unrecognized, and largely not practiced, but one would certainly recognize this role, if one would only stop and realize that unless one can be certain of what one might observe in well-defined astronomical contexts — that is, under such well-defined conditions — one could never be sure of any inference that one may draw from observations. Although I do not wish to go so far, there is an element of truth in an aphorism of Eddington's: "You cannot believe in astronomical observations before they are confirmed by theory." On the other hand, I agree that this is not practiced. And to some extent I am disappointed, at this stage in my life, that having practiced it for nearly 45 years now, it hasn't influenced others. To me, that's a

kind of disappointment that my attitude to science seems to be so little accepted, in the practice of other people.

[S. Chandrasekhar on the motives for his style of work.](#)

On the other hand, if you ask me, "Why have I practiced it myself?" Let me illustrate it in a different way. You have read Hardy's APOLOGY OF A MATHEMATICIAN? It's a marvelous little book. There's a reference in the end to a conversation which Hardy had with a friend of his, going by the Nelson/Column in Trafalgar Square. "If I had a statue on a column in London, would I prefer the column to be so high that the statue was invisible or low enough for the features to be recognizable? I would choose the first alternative."

The same motive works in science. I think one of the motives of science is to leave some kind of memorial behind oneself. And people can do that in a variety of ways. They can make discoveries and be remembered for that. But there is also a more modest role a scientist can play, and that is to assemble information and material which, in the long run, will

be helpful to others, and be of some permanent value — permanent in a relative sense.

I have chosen the later approach. All, I think, as a consequence of my first shattering experience in Cambridge. The idea that one's scientific life has to be motivated by the off-chance that one may make a great discovery, and be remembered for that, was too risky, too much of a gamble. I preferred the more modest approach of trying to do something— and I think, on the whole, it has worked to my advantage. Because if one is not stupid, then in the course of such effort you are bound to find a few things which people might even count as important discoveries. But the main emphasis in your life is to concentrate on producing as permanent a body of knowledge as you are capable of.

Weart:

I wonder, did you discuss this with anyone? Here you are, you're working on Equilibrium of Distorted Polytropes — Bohr sort of yawns at it — you must have discussed it with the other people round Cambridge at the time. Did you express it this way? Did you discuss it with people at the time, "Here I

am doing something which will be a real thing, which is solid?"

Chandrasekhar:

I could not have made it at that time. But what I tell you now is a gradual realization, as time has gone along.

Weart:

Somehow it's natural for you and you've gradually been able to articulate what you've been doing?

Chandrasekhar:

That's right. I don't think I could have said it in this way, at that time.

Weart:

I see. This was rather different from, for example, in 1929 Milne gave a famous lecture,* where he said the aim of mathematical physics was to build up a system, rather than to attack particular problems. What you say seems to me almost directly opposed to that. And of course, Eddington also was a system builder.

(* *Oxford Inaugural Lecture*. See G.J. Whitnow, "Milne", *DSB*.)

Chandrasekhar:

Yes. I may be wrong to say this, but I think — somebody said, "A person who says he is modest, is not modest." But I don't know how one wants to characterize it. My attitude to my work has always been motivated, not by any intent to make a discovery or produce a paper which will be considered important by my contemporaries — that, somehow, has never been a part of my mature scientific life. After I was 30 my idea has always been trying to find a place in science where I can do

work which would not be trivial, which would in time become a permanent body of knowledge.

Weart:

Don't you think there are other mathematical physicists and astrophysicists also who have some of this same feeling?

Chandrasekhar:

The only person, in the past, who seems to have been motivated in a similar way, as I can judge by reading his work, is Rayleigh.

Weart:

Ah, yes. But among the contemporary people, they seem to not take this attitude?

Chandrasekhar:

It doesn't seem to be. At least I'm not aware of anyone conspicuously following it.

Weart:

I have still a number of particular questions but I notice it's noon.

[Break for Lunch]

Weart:

Why don't we talk a little bit about theory and observation at Cambridge? So far, we haven't said anything about observations. Did you ever have any opportunity to do observations? Did you ever interact with the people who were doing observations?

Chandrasekhar:

No, I don't think I ever had anything to do with observations. But Eddington, of course, was very conversant with and sympathetic towards

observational work. In fact, he told me one very amusing story which I just remembered. He told me that one of the conditions for the Plumian chair in his time was that the candidate must have looked through a telescope at least 100 feet long. And he said that he had worked with Lord Ross's telescope in Ireland, and so was qualified at least on that account. Of course, he also told me that when he was chief assistant at Greenwich, one of his weekly chores was to wind all the clocks at Greenwich every Sunday morning. Well, having been through such chores, and having done his early work on proper motions and star-streaming, he was very sympathetic to observational work. But I myself have done no observational work, and indeed, I never thought that my professional career would be in the astronomical community, till I was appointed to an astronomical position in Chicago, in 1936.

Weart:

'36?

Chandrasekhar:

I received my appointment in 1936; but I joined the University in January 1937. I was visiting this country for three months in the winter of 35-36, from December 1935 till March 1936 and during the time I was there, Gerard Kuiper was a fellow at Harvard, and I believe that it was Kuiper who must have recommended me to Struve. And Struve invited me to come to Chicago in March of '36, and I came here. And before I left, Robert Hutchins had offered me the position at Chicago, as a research associate.

Weart:

Before that you had supposed that you would be doing what? You would go back to India?

Chandrasekhar:

I was a fellow at Cambridge, and the fellowship was to have run through till 1937. I fully expected that at the end of that, I would go to India.

Weart:

So there was no point in learning about large telescopes and so forth.

Chandrasekhar:

Yes. That was one of the reasons. And also, by that time, I had really decided that I would do theoretical work. But coming to Chicago, of course, changed my career, in the sense that Struve essentially made me in charge of graduate instruction at the campus here, and so I began to teach subjects like stellar dynamics and stellar atmospheres.

Weart:

We'll have to get back to that. I still am not entirely clear whether your case was exceptional at Cambridge, or whether in general the theorists did not talk very much with the observationalists. For example, Stratton. What was Stratton's attitude toward people who did only theory?

Chandrasekhar:

Somehow, the implicit felling there was — it's very difficult for me to say. I did know Stratton quite well, I used to see a good deal of him. He used to be particularly excited about his work on Nova Herculis, which came out in December of '35, I believe; and when that appeared he was very excited about the various stages of its development and he used to show the spectra to everyone; not only would he show it to me personally, he would also show it to me in company with other people, including Eddington. And there always used to be excitement as to what spectral lines were coming up in strength and what spectral lines were going down in strength, how the radial velocities were changing. But there is no gain saying, I was in an atmosphere of people who were far more interested in theory. My own social contacts, during my latter years in Trinity, were with the mathematicians. I used to see a good deal of them personally. And my contacts with Eddington and Milne were strong. At Oxford, I used to see Harry Plaskett, who was an observational astronomer and a solar physicist. Indeed, some of the problems I worked on in '35 and '36, when I was still in England, were related to some of Plaskett's

interests. For example, Plaskett was interested in the blanketing problem in the solar atmosphere, and I wrote a paper on that in which he was interested.* He was also interested in Wolf-Rayet stars.

("The radiative equilibrium of the outer layers of a star with special references to the blanketing effects of the reversing layer," on the distribution of the absorbing atoms in the re-revising layers of stars and the formation of blended absorption lines" (with P. Swings) MNRAS 97 (1936), 24-37.)*

Weart:

Yes, I wanted to ask you about Wolf-Rayet stars. There's a paper you wrote in 1934, and you thank him for the hypothesis that it's ultraviolet light [that ejects atoms from their atmospheres].

Chandrasekhar:

Yes.

Weart:

Is that how you got interested in Wolf-Rayet stars?

Chandrasekhar:

That was the origin of my interest. My personal friendship with Harry Plaskett got me also interested in the Zanstra theory; planetary nebulae; stellar atmospheres — in which I was already interested, but certain special problems; and particularly, I got interested already at that time in the discrepancy between Russell's extreme) hydrogen hypothesis and Unsöld's lower abundance color temperatures of stars— things which were to occupy me in the later forties. My initial interest came from talking to Harry Plaskett. The one observational astronomer with whom I had much contact was Harry Plaskett.

Weart:

What was he like?

Chandrasekhar:

Well, he's still living, you know. In fact, I met him during my last visit to England. We have retained a personal friendship during all these years.

I found him extremely pleasant personally, and very sympathetic to theoretical work. He was a great admirer of Milne, rather overly so, I thought — beyond reason. I believe in having friendships which are honest; just because he's a friend, you must not exaggerate his knowledge and ability; I believe in being a Nationalist about my friends.

Weart:

— you want to separate that from scientific questions.

Chandrasekhar:

Yes. And I thought that Harry Plaskett went too far in accepting everything that Milne said, in those days. Retrospectively, I must say that he has not

shown that degree of wisdom in astronomy as I thought he might have.

Weart:

I'm curious, you seem to have known the people at Oxford fairly well. I don't know very much about what astronomy was like at Oxford. You've given me a pretty good picture of Cambridge. Perhaps you could tell me things about Oxford also?

Chandrasekhar:

Well, my knowledge of Oxford largely derives from my personal friendship with Milne. I used to go to Oxford certainly once a term, and sometimes more often. Indeed, during summer of 1936 I spent three months in Oxford, with Plaskett and Zanstra in addition to spending three months with Milne during the fall of 1933 (Oct. - Dec.). I must have spent a total of six months at Oxford during my years in England.

I knew Knot Shaw, an active observer, who later went to Pretoria in South Africa, I think. The Southern Radcliffe telescope was there. The principal interest in Oxford was solar research. Harry Plaskett was interested in solar research, and so I got interested in the problems of the sun on that account. And Herman Zanstra used to come to Oxford quite frequently (he later became a professor in Amsterdam) and Zanstra and I became rather good friends. So I used to know the Oxford community more on the personal level, in the sense that I considered Harry Plaskett as a friend with whom I could go and talk at a level of equality. The same way with Milne. He was much younger, of course, than Eddington. We became personal friends very soon. Whereas in Cambridge, you see, Eddington, even though I knew him very well and would talk to him in an extremely natural way — he was always on Olympus as far as I was concerned. And Stratton was a very ebullient man. I remember very well, he was taking around a Spanish astronomer, trying to speak French, and talking about "Fe deux", (iron II) in the spectrum of Nova Herculis that he had found — and finally this man asked him, "What was the spark by which you got

that spectrum? And that sort of collapsed Stratton, because Stratton was talking about iron II lines in the spectrum of Nova Herculis! And (the visitor) thought it was a laboratory spectrum!

Weart:

In Oxford did you have any feeling about how the astronomers interacted with the physicists, about the balance or the separation between theory and observation?

Chandrasekhar:

I think it is safe to say that in England at that time, the relationship between astronomy as astronomy, and physics as physics was not very much — except through persons. Take Eddington, as a theoretical astronomer. In his book on the INTERNAL CONSTITUTION he says that R.H. Fowler was his referee for theoretical physics, and that C.D. Ellis was for experimental physics. C.D. Ellis was working on photoelectric effect, X-ray spectroscopy and so forth; and Eddington could learn from him about Kramers' opacity, and so, from the point of

view of learning about sources of opacity, he had to go to Charles Ellis. So it's quite clear that Eddington personally had contact both with experimental physicists, and with theoretical physicists, not to mention the experimental physicist, in Rutherford.

But of course, they were all Trinity College men.

Weart:

Right.

Chandrasekhar:

You see, so he got to know them that way.

Weart:

They all sat at High Table.

Chandrasekhar:

Yes. And then, take a man like Jeans. You know, Jeans became an entirely private person. Do you happen to know about Jeans?

Weart:

Not very much, no.

Chandrasekhar:

He was a professor in Princeton during the time when Woodrow Wilson was the president [of Princeton]. Woodrow Wilson somehow had the feeling that the best intellects were in England, and so he appointed a lot of Englishmen to the staff. Jeans was a professor and so was O.W. Richardson, so was Hugh Taylor.

In 1907 Jeans married Charlotte Tiffany Mitchell, daughter of Alfred Mitchell, explorer and traveler of New London, Connecticut. She was connected with

the well known Tiffany family of New York and apparently very wealthy.

Jeans remained in Princeton until 1909 and then returned to England presumably because he had elected to the Royal Society in 1907 and George Darwins retirement from the Plumian Chair. On his return to England, Jeans was appointed Stokes Lecturer in Applied Mathematics in the University of Cambridge. When George Darwin died in 1912, Jeans must have considered himself his logical successor - as indeed he was in many ways. But the electors chose instead. Eddington, Jean's junior by 5 years. Jeans apparently felt that he had no place in Cambridge and retired to a country villa (Cleveland Lodge) in Dorkins Surrey.

Weart:

Oh, I didn't know that story.

Chandrasekhar:

Jeans did. Some of his very best work — his Adams Prize, his stellar dynamics and cosmogony — were

written in the (period 1912-1919) in his private home: Cleveland Lodge, Dorking, Surrey.

Weart:

Did he come in also to Cambridge to talk with people?

Chandrasekhar:

No. No.

Weart:

You had no interaction with him?

Chandrasekhar:

No, I had no interaction with him. But he used to attend the meetings of the RAS in the thirties, and I used to see him at the RAS, regularly at a distance. He once invited me to his home in Surrey. I had a

marvelous time. He was a marvelous host. He met me in the car at the station, and took me to his house. He had an organ built in his house, and is known to be able to play most of Bach. You know, sometimes people are sarcastic about Jeans, and I always say, "Anyone who can play all of Bach's organ compositions cannot be a trivial person." Jeans was quite an aristocrat. I admired him then, and as time has gone along, my personal regard and respect for his work has, if anything, increased.

Jeans, of course, was entirely by himself. He had no contact with observational astronomy — except through his friendship with Hale. He was a research associate of Mt. Wilson. And he used to come to Pasadena and talk to Hubble and talk to Hale; that is why his book on astronomy and cosmogony is filled with pictures from Mt. Wilson. Because as a research associate, he could do that.

Weart:

Did you or the people at Trinity or Cambridge have much connection with the people at Mt. Wilson, or in general, with the big American telescopes?

Chandrasekhar:

Eddington, of course, was quite a good friend of the American people. Eddington was admired enormously in the United States. He was a foreign member of the Academy* — which Jeans was not. He had the Draper Medal and the Bruce Medal, from this country, and honorary degrees from a host of universities, including Chicago.

(* *National Academy of Sciences*)

Weart:

Did you people feel that you were quite up to date and in with things that were going on, with Hubble, and the pictures of galaxies and so forth? One doesn't see much cosmology or concern with galaxies in England.

Chandrasekhar:

Eddington worked on the universe.

Weart:

That's true. Among other people?

Chandrasekhar:

When Hubble came to England, I remember, Eddington accepted the expanding universe.

The fact that the expansion didn't quite agree with the age of the earth Eddington simply ignored and said, "Well, it'll all straighten itself out in time.

Weart:

Were these questions much discussed, for example, the age problem?

Chandrasekhar:

Oh yes. Eddington was very much interested in that.

Weart:

Were other people also?

Chandrasekhar:

Well, Jeans was. Of course, Eddington always set the standard. Whenever he gave a public lecture, the London TIMES used to report it fully. A controversy will ensure in which Jeans) Oliver Lodge, Larmor and others will take part. But Eddington always used to pride himself that, "I have never written to the London TIMES."

Weart:

They came to him. I see. Speaking of cosmology, this may be the right point chronologically to ask about the Letter you published in NATURE in 1937,* responding to Dirac's "Large Numbers" hypothesis. You come up with something of the dimensions of a mass, and you mention that you had noticed this some years ago but had been hesitating to publish — I'm quoting — "hesitating to publish

from the conviction that purely dimensional arguments will not lead one very far." Clearly there was a point at which you found this exciting.

(* *NATURE* 139 (1937), 757-58.)

Chandrasekhar:

Well, actually that Letter has a very curious history. I sent it as a personal letter to Dirac, and Dirac forwarded it to *NATURE*. He modified one or two sentences, to make it, suitable for publication. When I wrote to Dirac, it wasn't intended for publication.

Weart:

I see. I trust he let you know that he was going to publish it?

Chandrasekhar:

Oh yes, he asked me first if I would mind, and I said, no, if he thinks it is worth putting on record, it's all right with me.

Weart:

But there were ideas that you had had before?

Chandrasekhar:

Yes. But there is one fact, you know, which is mentioned there, which has never been seriously written about. I have myself written recently on it, recapturing some of my old ideas.

Namely that the combination of natural constants which gives one the white dwarf limit, provides the dimensions of a stellar mass and the fact that this is so is precisely the reason why modern theories of stellar structure are valid. This is in exactly the same way that the fact that the Bohr radius gives the correct dimensions of the size of an atom, provides also the reason for the validity of modern theory of atomic structure.

The principal point I wanted to make there was the significance of the combination of natural constants

which appears in the white dwarf limit. That was my principal reason. But the other coincidences, combinations of constants with dimensions of mass, well, I think those coincidences are still there.

Weart:

I see; as you say, if it has a significance in one case, it may have a significance in the other also.

Chandrasekhar:

Yes.

Weart:

One is really struck by this idea of Dirac's. I think ever since he came out with it, people have been somehow fascinated with it, even though they can't do anything with it. Where did this come from? Did it come from Eddington? Was he already talking about these things in the middle thirties?

Chandrasekhar:

Oh yes. He was talking about natural constants, yes.

Weart:

Is this where it comes from, do you suppose? Was it Eddington who began this, or did it go farther back?

Chandrasekhar:

Well, I would say that Eddington was the originator of these group of ideas. In fact, he was the first person, wasn't he, to point out that the ratio of the radius of the electron to its gravitational mass = 3×10 and he added "It's difficult to account for the occurrence of a pure number (of order greatly different from units) in a scheme of things; but this difficulty would be removed if one would connect it with the number of particles in the world"

(Mathematical Theory of Relativity, p. 167 (First Edition 1923). So far as I know, the isolation of this very large number was first done by Eddington, and largely under his influence, other people started to

go beyond him. Indeed, I think Dirac has stated that his interest in this arose from Eddington's ideas.

Weart:

It would be interesting to know, did Eddington ever talk about these ideas, the things that were coming to his "Fundamental Theory," in terms of where it came from, how he got into doing this type of physics or astrophysics, whatever it is?

Chandrasekhar:

Well, you know, all these men were playing for very high stakes. Eddington was. You must know the story, that when he was a young boy he was left alone at home, and when the family came back they asked him, "I suppose you found it very hard to be alone all this time." Eddington replied "Oh no, I counted all the words in the Bible." He quotes it himself. It is interesting that when he was young, he counted the number of words in the Bible; when he became a serious scientist he counted the number of particles in the universe!

Weart:

I see. I suppose perhaps the religious motivation may be found in those stories, also.

Chandrasekhar:

Yes.

Weart:

To get back to some of your own work — well, where are we? We're approximately to '35, and I think the new thing here that interests me is that you did the Wolf-Rayet business, and then in 1935, you come out with the idea that all stars may become dwarfs, if necessary by ejecting matter.*

(* *MONTHLY NOTICES* 94 (1934), 522; 95 (1935), 226.)

Chandrasekhar:

That's right.

Weart:

And you mention Wolf-Rayets. You mention that in some cases it would be novae: you mention that Milne had this idea. Is there more background to this?

Chandrasekhar:

Well, actually, the two were related. In fact, I was not only interested in the Wolf-Rayet stars, but also in a related work by N.A. Kosirev in which he suggested that high-temperature stars continuously eject matter. At that time, particularly in '35 - '36, I was really wondering very hard how a massive star was going to find a stable state. And somehow, the fact that very luminous and massive stars were ejecting matter seemed to me that perhaps one may seek a solution along these lines. And I was attracted to these problems also on that account.

Weart:

Is this why you wrote the paper on the Wolf-Rayet stars? Or did that come first?

Chandrasekhar:

Well, it came about the same time, because I wrote on the Wolf-Rayet stars in '35; I was also working on the white dwarfs problems at the same time. I was in Oxford and Plaskett told me about all these things about Wolf-Rayet Stars. I immediately said to myself, "My God, here we have probably nature telling us what is happening." So it was largely my contact with Plaskett which brought to my attention some of these things. And he told me about C.S. Beal's Wolf-Rayet stars having work flat-top contours. I got interested in these problems as an aside, in connection with my work on stellar structure at that time.

Weart:

I see. In a way, your work on stellar structure was still being oriented around the white dwarf problem.

Chandrasekhar:

That's right.

Weart:

Was it this idea that white dwarfs must be the end point, and therefore it's fundamental to everything?

Chandrasekhar:

Yes.

Weart:

I see.

Chandrasekhar:

I did a lot of work on white dwarfs at that time, which, because of the controversy with Eddington, I did not publish. I did some work on rotating white dwarfs; I did a fair amount of work on pulsating white dwarfs.

Weart:

Is this work that you later published?

Chandrasekhar:

No. The rotation part I published a short note on, but pulsation, I never did.

Weart:

Because you thought it was enough just to get the first thing accepted, without going on?

Chandrasekhar:

It was a very strange situation, because here I was convinced in my own mind that my work was right; but surrounded by a lot of people who thought that the work was all wrong. And after all, you must remember, I was in my middle twenties in those days, and I had to think about my scientific future. Was I going to be the one repeatedly telling others that I was right, knowing full well that the whole community against me? It seemed to me that even if I was right, as I never doubted myself, one's scientific future is not built on one discovery, no matter how important it may turn out to be. And I thought it was important for me to widen my knowledge. And so when I came to Yerkes, I started giving lectures on various other subjects.

Weart:

Right. And even at Cambridge you were publishing papers in various other areas.

Chandrasekhar:

Yes.

Weart:

We've said a lot about Cambridge. There's one other question I wanted to ask you, and I wondered, what sort of difficulties did you encounter because you were from India. Among the scientists, or perhaps in the larger community?

Chandrasekhar:

Well, in England, I had no problems.

Weart:

Because one does hear that some of the English adopted a racist attitude toward people from India.

Chandrasekhar:

Well, the English had a very interesting attitude to Indians. I'll tell you one very interesting story. I told you already the fact about Raman having been my uncle. He (Raman) told me once, when I was still in India, that during one of his visits to Cambridge in the twenties he visited Rutherford. Rutherford was talking Raman around and Raman told him, "Well, I see all the young are playing in tennis courts and so on — when do they work?"

Rutherford told him, "My dear Professor Raman, we don't want bookworms. We want governors for our empire." [laughter]. On the other hand, the British scientists were enormously interested in creating an Indian science. After all, Ramanjan would have died unknown, but for Hardy.

Weart:

Of course, some of them did go out and teach in Indian universities.

Chandrasekhar:

Yes. I am sure that during my time when I was in Cambridge, [H.J.] Bhabha was there, I was there, and Bhabha's experience was the same as mine: we were treated, if anything, with more consideration that we thought we deserved. I had racial problems later in this country. But not in England.

Weart:

I see. About your move to the United States, then — you've told me briefly how you came over, but I'm curious as to how American astronomy seemed to you, both when you were still in England, before you had come over, and also when you came here. You first visited Harvard. How did it seem to you, from Cambridge, both in terms of observation and in terms of theory and so of?

Chandrasekhar:

Well, I would say that in Cambridge, it did not seem to me at that time (but that was in part due to my

own ignorance) that theory was very well prosecuted in America. I do know that when Struve appointed me to Chicago, he told me, "We have no one here in this country in the tradition of Eddington, Milne, and theoreticians of that kind. And that is why I want to have you here."

I do know that when my appointment was announced, I understood later, there was considerable consternation the part of many people, as to why someone like me should have been appointed to an observatory.

Weart:

People at Yerkes or people in general?

Chandrasekhar:

People in general. Struve was very farsighted in these things. I suppose that isn't proper for me to say: it looks self-serving. But Struve did encourage theoretical work. He had personally as enormous admiration for the English astrophysicists, R.H.

Fowler, E.A. Milne and Eddington; and he ranked those men higher than Henry Norris Russell. He has told me that. He probably thought that I was in their tradition, and he wanted to have something here of the same kind.

But when I came here, first of all, I was completely and totally astonished that I would be considered a member of the observatory. I took that as a kind of requirement that I should get to learn more astronomical things — that is to say, to study stellar atmospheres more deeply, to study problems of dynamics more deeply. And so during my first three years at Chicago, I gave lectures on all these aspects of theoretical astronomy.

Weart:

Was this because you felt deliberately that you should be sort of a theorist at the service of these people?

Chandrasekhar:

I don't think I felt that I was needed to serve anybody; but I did feel that these subjects might give me scope for my own work. Particularly, I was anxious to give up stellar structure and go into something else. It came sort of naturally to me. I said, "If I can do things in stellar structure, why shouldn't I be able to do things in other branches of theoretical astronomy?"

Weart:

I see. You said you were very struck that you should be invited to Yerkes. From England, had Yerkes and also the other big telescopes seemed like very important places?

Chandrasekhar:

Sorry to say, I was not wise enough to think so. I knew that my time in Cambridge was coming up. I had only one more year. And when I got this offer from Chicago, I consulted my friends, particularly

Eddington, and Eddington told me that so far as he could see, there was not much scope for my getting a position in England. And he thought that going to America would be useful to me. I took his advice.

Weart:

Did people in Cambridge consider Cambridge to be the center of the astronomical world? Or did they regard the American observatories as extremely important?

Chandrasekhar:

I think by the time I was through my six years in Cambridge, I had developed a great deal of respect for American astronomy. There was Hubble, of course, whose work I had got to know; and Shapley, J.S. Plaskett, Adams, Bowen, Russell of course. All this seemed to me astronomy of a different kind, but astronomy certainly worth pursuing.

Weart:

OK. When you came over, you also married Lalitha Doraiswamy. You knew that you weren't going back to India at that point?

Chandrasekhar:

Yes. We knew each other already in the late twenties when we were students together. While we were not officially engaged, we knew more or less that we wanted to get married, and we sort of kept up a correspondence most of the time I was in England. And in 1936 when I had received this offer from Chicago, and knew that I had to come to this country in January, '37, I went to India in '36, for the summer; and while there we met again, and we agreed to get married and come over to this country.

Weart:

I see. You mentioned that she had been a physics student.

Chandrasekhar:

She was a physics student, yes.

Weart:

Which is why this book is inscribed to her. Then you got it back when you married her. That's the Sommerfeld?

Chandrasekhar:

That's right, ATOMIC STRUCTURE AND SPECTRAL LINES.

Weart:

Meanwhile had she been continuing in physics?

Chandrasekhar:

Yes, she was continuing in physics. First of all she was a teacher for some time. Then later, she had

joined the Institute of Science in Bangalore, hoping to do some research in physics.

Weart:

I see. We have these questions that we ask everybody — we're always curious: how do you think the fact that you're a scientist has affected your marriage?

Chandrasekhar:

I'll make one very broad, general statement. I am afraid that by giving to the pursuit of science the highest priority, one necessarily distorts one's personal life. That includes my marriage, in the sense that life has been very hard for my wife.

On the other hand, I do not by any means suggest that we have been unhappy with each other. No. I think I can say I've been as happily married as I could ever imagine having been married. And she has been of course an enormous source of personal

strength to me. But still, you know, to devote one's time exclusively to science, and give everything else a second place, does not contribute to one's personal growth. In other things it's the same, particularly after I took the editorship of the *ASTROPHYSICAL JOURNAL* in 1952. The *JOURNAL* took a lot of my time. But if you look at my scientific record, for what it is — I don't say it's good or bad or anything of the kind — if people did not know that I was an editor, I don't think they would conclude from my scientific record that I was an editor for 19 years because I don't think that people would find in my published record any difference, either before or after my editorship.

Weart:

It's very striking, in fact.

Chandrasekhar:

It doesn't happen by itself. It was a conscious decision. When I took over the editorship I said, "My editorship is not going to affect my science." which means that in addition to my editorship and

science, there was going to be nothing else. I had in fact time for anything else. When I was young, and when I was in Cambridge, I thought it would be marvelous to be able to read and memorize all the plays of Shakespeare. I still think so, but I still haven't done that. Well, was it worthwhile, to have pursued science in this single-minded way? I don't know the answer.

Weart:

Do you think that other scientists you've known have had similar effects on their personal lives?

Chandrasekhar:

I don't know. I'm afraid that by and large, have been a scientist entirely in myself. Of course, I have collaborated with my students; a number of students. The close association with my students has been the most valuable thing in life.

I may just make one remark, incidentally, that I worked with Fermi, and I worked with John von Neumann. People sometimes ask me: "Wasn't it marvelous?" And my answer is: "my association with my students personally has meant more to me than my association with these two people and the papers I wrote with them." It was nice, of course, to have known these great men, nice to have worked with them. But if you ask me, in the long range, who has influenced me more, I would say my students have influenced me more. That is one thing which is a different thing.

But I don't know, you see. I don't know enough about other scientists, my contemporaries, to make any judgement. And to the extent that I have read in the biographies of others, I've been unable to draw any conclusions about what their personal lives have been.

Weart:

It's often very difficult to find these things out, unless one happens to have, as with Karl Schwarzschild, we have his letters to his parents, where he tells sort of week by week what is going on

with him. But unless you have something like that — and even then, what can one learn? And so, your wife and you were married in England?

Chandrasekhar:

No, we were married in India. I went to India in July 1936 and we were married in September.

Weart:

Oh, you were married in India.

Chandrasekhar:

Yes. We came over to England in October, '36, and we stayed in England for the following three months. By the end of December, we came over to this country.

Weart:

Did you go straight to Yerkes or did you stop at Harvard?

Chandrasekhar:

No. Well, we stopped for a few days at Harvard.

Weart:

When you were at Harvard before, how long were you there?

Chandrasekhar:

Three months.

Weart:

Three months. I see. What was your impression of Harvard? You were there during its busy years.

Chandrasekhar:

I got to know a few of the people there. I got to know Gerard Kuiper very well, who later was my colleague at Chicago for many years, and a good personal friend. I got to know Fred Whipple (who has retired). I got to know Jerry Mulders, who was a fellow, postdoctoral fellow. I got to know Shapley, Menzel, Bok and Cecilia Payne Gaposchkin. But I'm afraid that there was a tremendous lot of sniping between different centers of astronomy at that time. For example, when I was offered the position at Chicago, I had a competing offer from Harvard.

Weart:

Oh, you did?

Chandrasekhar:

Yes. They wanted me to become a member of the Society of Fellows.

Weart:

Why was that? How did that happen?

Chandrasekhar:

I don't know. I was not involved with the politics. But I do know that the moment I was being considered for a position in Chicago, immediately there was a counter-offer from Harvard.

Weart:

It was because they knew that you had an offer from Chicago?

Chandrasekhar:

It might have been that. It might have been independent, I don't know. But anyhow, there was a counter-offer. Before I left the United States to go

back to England in late March of '36, Henry Norris Russell invited me to stay as his house guest —

Weart:

— in Princeton

Chandrasekhar:

In Princeton. And he tried to persuade me that I should not go to Chicago, but go to Harvard. But when I went to England and I consulted my English friends, Eddington very strongly advised me to go to Chicago. And effectively, I took his advice.

Weart:

Why was that, I wonder? What reasons did he give for your going to Chicago?

Chandrasekhar:

Well, Eddington probably thought that Struve was somebody that he knew, and he thought the fact that Kuiper was being invited at the same time and that Stromgren was also being invited meant that I would be with a relatively younger group.

Weart:

So it wasn't so much anything about Chicago itself.

Chandrasekhar:

Well, he knew [the University's President], Hutchins, and Eddington told me that Hutchins was a very strong and upright man.

Weart:

I see. Were there any negative feelings about Harvard? Or did you perhaps get any while you were there?

Chandrasekhar:

I had a feeling that there was a — nothing very specific, but I sensed in a way, that the general attitude towards Harvard was not wholly one of admiration.

Weart:

And while you were there, what sort of feeling did you get about it as a place? Especially coming right from Cambridge [England].

[Tape # 4 (Side 7)]

Weart:

A newspaper clipping: "Hutchins finds professors no moral lights worse than most people, he says."

Chandrasekhar:

Look at this.

Weart:

FROM THE NEW YORK TIMES (Ca. 1968)

"Hutchins cited incidents that occurred during his term as UC chancellor. He headed the university from 1929. The medical school of the university 'violently resisted admitting Negro students,' Hutchins asserted. On another occasion, he said, the chairman of a scientific department told him the university could not appoint a leading theoretical astronomer to its faculty because he was an Indian and black."

Weart:

Indeed! Did you hear about that at the time?

Chandrasekhar:

No. I didn't hear about it at the time, but I was a little surprised that when I came to Chicago in the March of 1936 and when Struve wanted to offer me the job, he took me to see Hutchins. The President of the university, does not normally see an assistant professor who is to be appointed. Then Struve later went to see the dean. But I stayed outside, while he went to see the Dean.

Weart:

At the time you were just puzzled by it?

Chandrasekhar:

Somehow, it made no special impression — I remembered the fact, but I did not draw any conclusion. But later on, I had reason to suspect that something of that sort was going on. In fact I learned later it is not connected with this story but it is interesting any way. Zachariasen told me this story (he was the chairman of the physics department):

Dean Gale who was a professor of physics particularly interested in optics, was familiar with Raman's work, and Raman visited Chicago as Compton's guest in the late twenties. Compton knew Gale's interest in Raman's work, and so he had suggested to Gale to join himself and a guest of his at lunch at the club. When Compton and Raman were sitting in the club, Gale came in, saw the complexion of the man sitting next to Compton, turned around and walked away. Apparently Gale's predilection in this matter was known to others, but I did not know. In fact, I never met and talked to Gale, even though we overlapped in the physical sciences division, for several years. It's a remarkable story.

Somebody called me from the NEW YORK TIMES and asked me what I thought of this. I said, "Well, it simply shows that the University of Chicago was 30 years ahead of its time." I met Hutchins last summer when I was in Santa Barbara, and he recalled this remark of mine and said he was very pleased.

Weart:

I see. Did you encounter racism already when you came over to Harvard?

Chandrasekhar:

Not during the first three months I was there. But when I told my friends in India, particularly my father, they were all against my accepting the position in the United States, because they all felt that I would confront racial prejudice in this country. In fact, one of the arguments that was used by Russell to persuade me to accept Harvard, in preference to Chicago, was that in Chicago I would meet racial prejudice, whereas. in Harvard I won't. That's what he told me. I did not meet it at the university. But a year or two later, when my wife and I went to Columbia, we went from hotel to hotel, couldn't get any admission, and the only way we could finally find one was to call the astronomy department at Columbia and talk to an astronomer there. He met us at Grand Central Station, and found a place for us to stay. And several times later, I had similar difficulties.

Weart:

Did you encounter further difficulties inside the university community?

Chandrasekhar:

None whatever.

Weart:

It was strictly difficulties —

Chandrasekhar:

Outside.

Weart:

In the community outside. I see.

Chandrasekhar:

I mean, not only did I not find anything in the university, or universities in general — I would say that the treatment I have personally received from various universities in this country, including most of all Chicago, is beyond all praise. Indeed, I was offered professorship in Princeton in 1946, when Russell retired, but I declined.

Weart:

I see. Princeton is not noted as a place where —

Chandrasekhar:

— no, but I was offered a position at Princeton University, when Russell retired, and I declined it. It was because I declined it that Lyman Spitzer and Martin Schwarzschild were appointed there at that time. So I don't think that I can point at anything prejudicial against any of the universities I have been acquainted with, in all my 40 years in this country.

Weart:

I see. I wanted to ask you a little about Henry Norris Russell, and there's a particular reason for that. David DeVorkin is very interested in him, and in fact he's interested in doing a biography, possibly, of Russell (but maybe I shouldn't say this, it's still something he's considering). Anyway he'd like to know anything you know about Russell — how you encountered him, how he seemed to you, what his reputation was in various places.

Chandrasekhar:

Well, let me say this — Russell was one of the most enthusiastic astronomers I've known. Enthusiastic about everything in astronomy. Intensely interested in what other people were doing and interested in encouraging young people. In some ways, it is surprising that a person with so much interest in others should have had so few students in his life. I remember his once describing his former students, how Harlow Shapley came to him. In fact, he said,

"God sent me to him, when I had all that work to be done."

Then, Theodore Dunham, Donald Menzel, and Lyman Spitzer — I think that about covers all his students.

Weart:

Not a bad list, even so.

Chandrasekhar:

But still, for a person who was a professor in Princeton for 40 years?

Weart:

Yes. That's quite striking. Why do you suppose that was?

Chandrasekhar:

Well, astronomers as a rule don't take students. Shapley — who are his students? Hubble, Merrill, Campbell, Eddington, Jeans? Or take some modern people: Sandage has had no students. Somehow the idea of developing a school is simply not known among the astronomical community. There may be a variety of reasons for that.

This is perhaps a story which illustrates Russell's character: I was spending one quarter in Princeton, in 1941, the fall when Pearl Harbor happened.

Russell used to give lectures, usually with three or four people. Gradually the people would disappear one by one and I would be the only person left; Russell would then come sit next to me, and talk to me.

At first it was astronomy; after a while, it used to turn to other matters. And then he started talking about his admiration of Eddington, and the fact that he didn't think so well of Jeans. It came out in a curious way. He was praising Eddington, for his having received the Order of Merit, and suddenly,

"Well, I notice Jeans has now an Order of Merit. That must have taken a lot of pleasure out of Eddington."

He sat quietly for a long time; and then he said, "Well, I suppose it is not a crime for a person to live on his wife's wealth." — meaning Jeans retiring to Dorking Surrey (presumably on his wife's wealth).

Weart:

Interesting. The lectures were such that the other students had trouble keeping up?

Chandrasekhar:

Well, he started at 10 and used to go on till 12:30.

Weart:

I see.

Chandrasekhar:

So people simply couldn't stay there all the time. They had other things to do.

Weart:

I see. Did he ever talk about personal things?

Chandrasekhar:

Oh, he has talked about personal things. I mean, he talked about his disappointment — I remember very well, I don't know in what connection, we were talking about something about Coupling, and he said he had the Pauli Principle right in his hand and let it go, you see.

Incidentally, Russell made a tape, when I was at Princeton, which Lyman Spitzer had arranged: a tape of Henry Norris Russell reminiscing. It must be in their files.

Weart:

Oh, is that so? At Princeton? We'll have to look for it.

Chandrasekhar:

Yes. There is a tape which Martin Schwarzschild and Lyman Spitzer arranged to have made; Russell talks about his relations with Pickering, and how, when Pickering retired, he was offered the directorship at Harvard but wouldn't take it. And how he was extremely glad that Shapley took it.

I had a fair amount of contact with Russell. But at my time already he was interested only in analyzing spectra and things like that. But he was also tremendously interested in what other people were doing.

Weart:

How did people regard him? How was he considered by people in the United States?

Chandrasekhar:

All the people whom take for example Struve — Struve had the highest, highest regard for Henry Norris Russell. In fact, the University of Chicago gave him an honorary degree in 1941, at its 50th anniversary.

But I have a faint suspicion that he was not so well received on the West Coast. I heard a marvelous story Redman told me, about one occasion, when Adams was the director [of Mt. Wilson]. Russell was a great friend of Adams — there's the famous Russell-Adams calibration — and Redman told me that Russell came to see Adams, and went inside Adams' office, and for nearly two and a half hours, he heard Russell haranguing him. And finally they both emerged Adams looking very low, and Russell telling Adams, "Adams, you are too narrow."

Russell was tremendously enthusiastic. He used to talk non-stop. Tremendously interested in things.

Weart:

Must have had a very strong personality.

Chandrasekhar:

Of course, for him, ICS meant the Bible. (ICS is the INTERNAL CONSTITUTION OF THE STARS).

Weart:

So he didn't have a particular interest in your white dwarfs either?

Chandrasekhar:

Well, he was very dubious about it. For example, he never included it in any of his accounts on the subject.

Weart:

Was it accepted at Yerkes, or I should say at Chicago?

Chandrasekhar:

Oh, very well. Because Struve set the style, in my time.

Weart:

In terms of your work on degeneracy and so forth, did you discuss it in the thirties?

Chandrasekhar:

I discussed it with Russell but he was very non-committal.

Weart:

And with Struve and so forth, did you discuss it with these people?

Chandrasekhar:

They never knew anything about

Weart:

I see.

Chandrasekhar:

I'll tell you something — I don't know to what extent it ought to be on public record —

Weart:

— you can always cut it out later on.

Chandrasekhar:

One of my colleagues, in the middle sixties, was giving lectures on under graduate astronomy; and Jean Hopkins, who was a copy editor on the JOURNAL, (and still is) attended those lectures. And after the lectures she went up to the professor and asked, "What is Chandra's Limit, I hear about all the time?" "Oh, I don't know anything about it."

And A. Trautman, who is a very distinguished relativist was visiting me in 1971. He was invited with a group of astronomers, and members of the faculty were there. Trautman asked one of them, who was interested in galactic structure, "What do you think that the existence of this limit for white dwarfs implies for stellar populations and galactic evolution?" The man said, "I've never been able to understand Chandra's limit."

And I remember asking one of my younger colleagues, "Do you know about it?" He hemmed and hawed, "Well, I haven't read it properly." I said, "Do you really mean to say that what was accessible

to an undergraduate student in 1930 is not accessible to you today?

And take Struve. In one of his early books on popular astronomy, he mixes up the white dwarf limit with the other one which is connected with the maximum mass of the isothermal core. When a star burns its hydrogen.*

(* *The "Schonberg - Chandrasekhar" limit, as put forth in 1942. See APJ 96 (1942) p. 161.*)

Weart:

Yes. I remember when I was studying astrophysics, I had trouble knowing which limit was —

Chandrasekhar:

— was which.

Weart:

There are various limiting masses that people talk about, and I never could figure out which was —

Chandrasekhar:

Well, after all, the fact is that astronomers — this is not a reflection on the persons involved, it's simply a reflection on the kind of education the astronomical community has — the fact simply is that the professional astronomer is not acquainted with these things. And indeed, not so long ago, I asked one of the graduate students coming up for his Ph.D. oral a question on the white dwarfs. And I was asked to shut up by one of my colleagues, because I was asking questions in which I was interested. What does come out is, really, that the astronomical community somehow was not receptive to theoretical ideas derived from theoretical physics.

The situation has drastically changed, during the sixties. Particularly with the development of X-ray astronomy, and the theory of pulsars. Now people realize that all this is not so far-fetched. But right up to the early sixties, I don't think they accepted it.

To come back to your question as to how I was treated — I think the answer is the following: As far as I am concerned, if I ask myself: was I appreciated? How do I judge it? Do I judge it from the awards I have received? Certainly, the Bruce Medal came to me when I was 40. The RAS gave me the Gold Medal. I was elected to the Academy, and so on. There is clearly nothing in the record which suggests that I was not appreciated. And personally, certainly, if I consider appreciation from that point of view, I certainly was. As I have said, very much more than would have ever thought I deserved. But on the other hand, if you ask me if my particular work was understood, as a relevant part. I'm afraid there was an enormous time lag, between when the work was done and — when it got absorbed in common knowledge. But that itself is not surprising. Sometimes a work of this kind takes some time to get into the main stream.

So everything I've said simply adds up to the following, that the problems or astronomy, during the time I was active, excluding the last 10 or 15 years — theoretical work never became an integral part of astronomical development and astronomical

appreciation (in a genuine sense, not in terms of personalities).

Weart:

Specifically, at Chicago and Yerkes, in the late thirties and after, for a long time you were the only theorist? You regarded yourself as the only person doing theoretical — ?

Chandrasekhar:

— because Stromgren left very soon afterwards. But he came back later. I was the only person.

Weart:

When Stromgren was there — ?

Chandrasekhar:

Stromgren came in '36, fall, and he left in '38, spring. So we overlapped for a year and a half.

Weart:

So did you feel that you had some support? Was this when you first met him?

Chandrasekhar:

No, I had met him in Copenhagen.

Weart:

Already in Copenhagen, I see.

Chandrasekhar:

I had considerable contact with Stromgren that year and a half when we were together at Yerkes. My contact with him was very slight afterwards. But during that year and a half, we had constant relationship.

Weart:

What sort of other relations did you have, whom did you talk with about theory? Physicists?

Chandrasekhar:

Kuiper was very interested in the theory of white dwarfs, because he was working white dwarfs. He was one of the few astronomers who wanted to integrate theory and observations. So I had considerable contact with Kuiper. I think I would say that Kuiper was about the only person on the faculty with whom I had contact during that period. On the other hand, starting in the early forties, I went on into other areas, where the contact was not very great in astronomical terms.

Of course, the whole character of my work changed in 1950 or so, because at that time I changed my field of interest to hydrodynamics, hydromagnetic stability and I got into contact with the physicists on the campus.

Weart:

And the mathematicians?

Chandrasekhar:

Well, the physicists, Fermi, and Wentzel were the principal persons; and then I came in contact with the geophysicists like Dave Fultz who did experiments for me, in some of these convection predictions.

Weart:

But up until that point, had people in the astronomy department in general had much contact with the physicists?

Chandrasekhar:

No.

Weart:

They were sort of off by themselves.

Chandrasekhar:

Yes.

Weart:

I'm not sure what kind of working conditions you had. Where were you located physically?

Chandrasekhar:

I was located at Yerkes till 1964. However, after 1946, I used to come down to the campus once a week to give lectures here on the campus. In the middle fifties, we had an apartment in the city here, so that I could come and stay over night. So we used to spend part of the week here, Chicago, and then the rest of the week at Williams Bay. Then by the early sixties it became clear that there was no point making this tremendously exhausting drive back and

forth every week, and so we came over to Chicago permanently.

Weart:

I see. During the thirties and the early forties, did you get down much or did people at Yerkes in general get down much to Chicago, to lecture, to talk?

Chandrasekhar:

No.

Weart:

So in fact, there wasn't much opportunity even to interact with the physics department?

Chandrasekhar:

That's right. I had some contacts, in spite of it. I used to know Carl Eckart quite well. Particularly during the war, when Wigner was here, I used to come down once a week to Chicago to talk to Wigner.

Weart:

Just to talk to Wigner?

Chandrasekhar:

Yes. He had a lot of influence on me. I was interested in the theory of H^- and at that time, and I could talk to him about it. He is very knowledgeable, even though it was sometimes difficult to draw him out. I used to persist, and I got a lot of encouragement stimulation, from talking to Wigner here in Chicago during the war.

Weart:

I see. One thing I never quite understood about Yerkes and that is, how it maintained a teaching program? How did it teach the undergraduate level?

Chandrasekhar:

We didn't teach undergraduates.

Weart:

There were separate people here [in Chicago] teaching.

Chandrasekhar:

No, there was no undergraduate teaching.

Weart:

There was no undergraduate teaching in astronomy?

Chandrasekhar:

Except when Bartky when he was here, he might have lectured. Walter Bartky was in the mathematics department and also in the astronomy department. He was a pupil of Macmillan. And he used to teach some undergraduate astronomy. In fact, he has a book on astronomy (Highlights of Astronomy?) an elementary textbook on astronomy which Bartky published and from he used to teach. But he stopped doing that in 1940-45, during the war and later.

Weart:

Now, you were doing graduate instruction. In fact, you were put in charge of graduate instruction.

Chandrasekhar:

I was in charge, and if you look at the old catalogs of the university, you'll find that I taught anywhere between one-third to half of all the courses that were taught at that time.

Weart:

How did it happen that you were put in charge of that in the first place?

Chandrasekhar:

Well, Struve put me in charge. In the middle fifties the department decided that the kind of things I was teaching was too theoretical, and they changed the style of teaching. But up to that time, the faculty was small, and I was the only person willing to do the teaching, and I did so pretty well most of the teaching. And of course, you know, my students of those times — three of them are in the National Academy: Don Osterbrock, Arthur Code and Guido Munch. Jerry Ostriker was a student of mine over the protest of the Department in early sixties; and he is also in the National Academy.

Weart:

I see. You must have been spending quite a lot of your time in teaching then?

Chandrasekhar:

But I integrated my teaching with my own research.

Weart:

You would be teaching theory, and then you would be working out?

Chandrasekhar:

For example, I started teaching stellar atmospheres. I went on into working on radiative transfer and then on to the problems relating to Hydrogen.

Weart:

I see. Would it be possible to say how much time you were spending on teaching, how much on research, how much on administration and other matters?

Chandrasekhar:

I would say that during the first ten years of my stay in Chicago, I did not consider my teaching and research separate. I was doing graduate teaching, and since I was not a trained astronomer before, I started teaching astrophysical courses like stellar atmospheres, stellar dynamics, interstellar space, galactic structure and all these things, and my research was integrated into my teaching. So that many of my students used to attend the lectures year after year, because each year it was different; I was doing different things. For example, during the middle forties, when I was lecturing on stellar atmospheres, Lawrence Aller, who used to be in Michigan, used to come down to Yerkes to hear my lectures. Because I was teaching many things that were never taught elsewhere. So, while I gave a lot

of lectures in those days, on everything I lectured, I was doing research.

Weart:

You practically published what you'd been lecturing
—

Chandrasekhar:

Lecturing and doing research in those years were the same activity. But then, the only administrative thing I ever did was during the year when Struve left Chicago and went to Berkeley, and Stromgren was yet to come: I was acting chairman that year (1950). But apart from that, I have add no administrative responsibility of any kind.

Weart:

I would think that being in charge of the graduate program would have a lot of administrative responsibility.

Chandrasekhar:

But graduate students are not very many. Practically all the students that came there worked with me. So I had effectively no administrative problem. On the other hand, in 1950-51, I expected the character of my life to change discontinuously.

Weart:

Was it because of the chairmanship?

Chandrasekhar:

No. Because I was changing my field of interest into hydrodynamics and hydromagnetics

Weart:

We'll have to come back to that — What about your colleagues at Yerkes? What kind of hours did people keep? Did they spend much time teaching?

Chandrasekhar:

On the whole my colleagues spent very little time on teaching. They did some, but very little. They never gave what I would consider systematic courses. But during the forties, some of the best things connected with the university were done. Struve was at the height of his career, doing marvelous work on peculiar stars; Morgan's best work on the spiral structure was done during those years; Kuiper in the course of one year discovered the inner satellite of Uranus, the outer satellite of Neptune, carbon dioxide on Mars and the atmosphere on Titan. Hiltner's discovery of inter stellar polarization was also at that same time and we had visitors from Europe. Minnaert spent a year with us, Oort spent six months with us, and Unsöld was there for some time. And Van de Hulst was a postdoctoral fellow working with me. Actually I would say, from 1946 to about 1949 was the period in which, certainly retrospectively, the department reached its heights. I'm afraid it eventually declined, for a number of years, but I think it's back again on the top now.

Weart:

We'll have to talk about that too when we come to it. Particularly, sort of your early impressions-you must have compared it with Cambridge. Did it seem to you a place where people worked very hard, where things were organized differently from Cambridge, where professors and students had a different kind of relationship from what they had in England?

Chandrasekhar:

Well, you have asked this question several times. But I want to explain my state of mind in those days. I always felt that I was in Cambridge and also here in the United States on sufferance. In other words, to me, the external circumstances did not play any role on my life. For example, I was a fellow of Trinity for four years, and the fellows meet and vote on various things. At meetings of the college council, the college foundation. And in all my years in Trinity, I never voted once, even though I attended

all the meetings. Somehow, I thought that it was not proper for me to contribute to the decision by voting.

I did not feel differently when I came to Yerkes. It did not seem to me that I was there for any other purpose except for doing my work. To take an example [telephone rings] — when I came to Chicago and joined Yerkes, all my colleagues had full-time assistants. I never had any, till 1944; and the thought that I was discriminated never even occurred to me.

Weart:

I see, and you hadn't even asked.

Chandrasekhar:

I wrote all my —

Weart:

— I hope I'm not getting in the way of your phone calls.

Chandrasekhar:

No, no that's all right, it's not very important anyway. I was saying that, for example, till 1943, every manuscript I wrote, I wrote longhand. And my first two books were written and sent for publication on longhand; my entire STELLAR STRUCTURE and STELLAR DYNAMICS, and the article in the REVIEWS of MODERN PHYSICS to mention three — they were written longhand and sent to the publishers longhand.

Weart:

— in longhand, I see.

Chandrasekhar:

In fact, the archives that you saw in the Requisitions Library contains the handwritten manuscripts of my STELLAR STRUCTURE AND STELLAR DYNAMICS. I did not have any assistants, and all my letters I wrote longhand. And it never occurred

to me that some discrimination or something was made. I do not know whether it was discrimination or not, but the fact is. Now, these things sometimes puzzle me but then I never asked myself the question, "Is this functioning the way it ought to function?" I never did. So, looking back on those times, the thing which surprises me is how little I considered as relevant to my career what was going on around me. I got something positive — well and good. If I did not get anything — well, it made no impression on me. So this is an essential fact.

Weart:

I can understand this. And particularly, being a theorist, you don't have the same interest in the institutions that an instrument man would.

Chandrasekhar:

So long as I was allowed to do my work, and nobody interfered with me, and I had full access to publishing what I wanted to publish then I had all I wanted. Struve in fact made me an associate editor

of the ASTROPHYSICS JOURNAL in 1944, and so I could essentially publish whatever I wanted the ASTROPHYSICAL JOURNAL — so, I thought I had everything I wanted. The rest didn't matter to me.

Weart:

Tell me, what was Struve's style as a leader?

Chandrasekhar:

Struve was a very extraordinary man. He was autocratic in many ways, but I never had any personal difficulties with him myself. He had only one interest and one concern, namely, that astronomy should be developed and pushed to the maximum that was possible.

When I first saw him, he was a very distinguished man, already in his mature period, and the staff which he built was all in their twenties, and all of us

looked up to him and never questioned his authority or his judgement.

But when the middle 1940's came along, even though as far as I can remember my attitude to him hadn't changed — he must have felt that the difference between him and his colleagues was diminishing; and he used to sometimes behave in a very funny way. For example, I remember we used to give him our annual reports. He had assembled them all and after one year, (it must have been '46 or '47) he called a meeting of the faculty, in which he had the reports of all the people in front of him, and he said he wasn't satisfied with the progress that each of us had done. Then he passed around the SCIENTIFIC MONTHLY, in which there were pictures of some natives in Africa with elephantiasis, with enlarged legs and limbs. He passed the picture around and said, "I have to deal with a faculty in which each member has elephantiasis of his own capabilities." That was a very rude thing to say and you should remember that the faculty at time included Herzberg, Hiltner, Morgan, Kuiper, Greenstein, Henyey and myself.

Weart:

So long as he said it to everybody at once —

Chandrasekhar:

He was tremendously interested in the development of astronomy, very anxious to serve it the best way he can. His ideal was Hale.

Weart:

Is that so?

Chandrasekhar:

Yes, his ideal was Hale, and he imitated his life, I think even consciously. You know, Struve was responsible for building the MacDonald Observatory at Texas.

Weart:

Yes, I was going to say, it sounds like this must have something to do with the origins of McDonald [Observatory].

Chandrasekhar:

Yes. And he wanted very much to be treated the way Hale was treated in the latter part of his life. It was his great disappointment to him that he felt that he was not so treated. Struve was a man given to great moods of depression, but in his happier times, a marvelous colleague and certainly, he ignored all irrelevancies and concentrated on what he cared for) namely astronomy. I don't see how one could have a better director. He was intensely interested in astronomy. That was the only thing which mattered to him. And he looked at all his colleagues in terms of what they were doing for astronomy. And he was sensitive to his own place in astronomy, perhaps even in a morbid way.

Weart:

I suppose that could be in part because of his ancestors and so forth. That must have had a powerful effect on him.

Chandrasekhar:

Yes.

Weart:

You know, speaking of Struve, Stromgren, yourself and so forth, all people coming over as foreigners, and then of course there were the refugee physicists, we mentioned Wigner, and others. I wonder what effect you think this may have had on the development of American astrophysics and astronomy, in the thirties and after?

Chandrasekhar:

I think obviously the effect has been very profound. Bethe's carbon cycle of 1939 — the nuclear

reactions that he postulated at that time — has become a central part of astronomy. And —

Weart:

— but in terms of what it may have done to the American approach to astrophysics and American institutions.

Chandrasekhar:

I don't want to say "American." Because the difference between scientists in one country and another is less than the difference between scientists and the rest of the community in any country. You understand that. So I don't want to use the word "American" either in the chauvinistic sense or in the derogatory sense.

Weart:

Let me put it in this way. Bethe presumably could have done the carbon cycle had everybody stayed in

Germany. And so forth. But what difference may it have made to the development of modern astrophysics, that so many people came over to this country?

Chandrasekhar:

I think, purely in an objective way, I'd say the following. The development of astronomy during the 19th century, made a very big change. First they measured the parallaxes and proper motions; then they started to measure radial velocities; then they began taking spectra. Well, clearly, one had accumulated an enormous amount of information. And so astronomy centered on accumulating data.

While this is an essential component in the progress of astronomy, it also meant that astronomers got separated from the rest of the scientific community. Take a man like Aitken. He goes to Mt. Hamilton, spends his whole life measuring orbits of binary stars. Or take someone like Campbell, who said that everybody in his observatory had to measure radial

velocities. Or people who look at variable stars — everybody has to draw light curves, thousands of them, and they take pride in the fact that a millionth setting has been made. One can be sarcastic about all this but one shouldn't be, because the core of astronomy depended upon getting these observations. But it also meant a change in the character of their lives. And principally in America, because here they built all these observatories, it meant that the concentration of the astronomical community was in this data-gathering process. A person can start at the beginning of his life to do one thing, and he does the same thing the rest of his life. Whereas the astronomical people who came from Europe were not in this tradition. Like Bethe or me or Martin Schwarzschild. And consequently, this group played a role in the progress of astronomy, in the sense that astronomy now had to change its character, had to go into understanding what the observations meant. And the American astronomical community, up to the thirties, forties and even the early fifties, had been built around the enormous job of gathering data.

It just coincided, historically, that at the time that astronomy needed a change of pace, it was fortunate

that people with that appropriate training happened to be here. So I think that to say that these people made American astronomy is false — if it means a chauvinism, then it is wrong. The point is, sometimes important things happen because of historical coincidences. And there was this historical coincidence, in which the progress which could probably have taken place in time on its own initiative was helped by historical circumstances. Therefore, if one writes the history, one would find that people with a different backgrounds and a different training helped the growth of astronomy along directions which astronomy was waiting for, and which probably would have taken place in any case.

It is still true that the fundamental centers of astronomy were devoted to gathering data. But astronomy canon have developed entirely only in that direction; it had to change. The change took place, and it was accelerated perhaps by the kind of people who were here at that time.

Weart:

That's interesting. The kind of courses that you taught, for example, must have been quite different from the kinds of courses that had been taught before.

Chandrasekhar:

That's right.

Weart:

You must have had some picture of the other teaching. How did the teaching here at Chicago compare with the kind of teaching that had gone on at Trinity, in terms of the content, in terms of the situation?

Chandrasekhar:

Well, I would say that the kind of teaching in astronomy which was initiated in the thirties and forties was different from what was in England at

that time, and from what was in this country prior to that. Because in England, there was no program in astronomy as such. During the years I was in Cambridge, there was a course on the internal constitution, which Eddington used to give; a course on relativity; a course on combination of observations and Stratton used to give one course every term on astrophysics and occasionally a course in spherical trigonometry.

Whereas, the course which I started to give here, which very soon were repeated in many other places — like a three quarter course in stellar atmospheres, a three quarter course on stellar evolution — these are completely filled-out programs. And now the programs in astronomy which are given in Europe are modeled after what has been done here.

So I think in a sense, due to circumstances, the character of astronomical teaching changed, in this country. Changed first, and later changed also in Europe.

Weart:

What was it like before? Of course you weren't here so you wouldn't have had too much contact.

Chandrasekhar:

There were no courses in astrophysics given at all.

Weart:

None in astrophysics?

Chandrasekhar:

Graduate teaching, the way it was given in '37, was started completely anew.

Weart:

Did you encounter any resistance?

Chandrasekhar:

No. Struve was the boss and Struve said, "You have a free hand."

Weart:

I see. What about the students? They must have been a bit stunned by it at first?

Chandrasekhar:

Yes, in the first year or two. But they got along.

Weart:

Let me ask you a bit more about the social character of Yerkes, because I really don't know much about it. For example, was there a regular seminar that people went to?

Chandrasekhar:

Yes. In fact, Struve asked me one of the first things, was "Chandra, we must have a weekly colloquium; and that is your responsibility.

Weart:

A theoretical colloquium.

Chandrasekhar:

No, a weekly colloquium. I started running it, and gave the first colloquium I organized, the number 1. And I kept it on myself, up to about a thousand. Which it reached in 1962.

Weart:

Had there been colloquia before?

Chandrasekhar:

Apparently randomly at intervals, but after Struve asked me to do it, we had it every week.

Weart:

Why did Struve pick you to do all these things?

Chandrasekhar:

Well, according to him, everybody else had real jobs to do — namely, to go to the telescope and observe. I was there sitting in the office, never worrying about whether it was clear at night or not.

Weart:

I see. You didn't have to get up in the middle of the night, I suppose.

Chandrasekhar:

Yes.

Weart:

So how did you organize this colloquium? How was it run?

Chandrasekhar:

First of all, I tried to arrange that all the faculty participate: one week after another. And then I used to get people from the physics department on the Campus and an occasionally when visitors passed through, I used to have them talk.

Weart:

I see. But it hadn't actually been done that much before?

Chandrasekhar:

No.

Weart:

Aside from the colloquium, in general, where and how did the staff exchange ideas about research?

Chandrasekhar:

Well, during the first few years, Struve for example used to attend all the other courses that were given.

Weart:

You mean, come and sit in?

Chandrasekhar:

That's right. And I used to sit in on his courses.

Weart:

Were all the courses on this kind of an advanced level, that it would be profitable?

Chandrasekhar:

To some extent, yes. And of course, I tried to learn from others, and they tried to learn from me.

Weart:

I almost get the impression that the courses, in some sense, were like an ongoing colloquium.

Chandrasekhar:

That's right.

Weart:

Would Struve comment perhaps while you were lecturing?

Chandrasekhar:

Sometimes. Yes.

Weart:

Were there other places where you would tend to meet people, in the observatory? After hours? Much socializing?

Chandrasekhar:

There was some socializing, yes. But the observatory was the center, and most of the people used to work in the evenings. I remember, there was a visitor from Australia who came and I met him some years later. He said that when he first came to Yerkes, ('46 or '47) he arrived at the observatory at 7:30 one evening, and he was shocked to find that all the faculty were in their offices. Struve was there, Kuiper was there, Morgan was there, I was there, and everybody was there.

Weart:

That's very different from the British way, isn't it. Isn't Rutherford supposed to have locked up the Cavendish in the evenings or on holidays and so-forth?

Chandrasekhar:

That's right.

Weart:

I was just up there yesterday, of course, and I saw the "battleship" dormitory where the graduate students lived and so on. I almost get the impression of a semi-monastic existence, everybody very concentrated on astronomy. Is that the feeling that one got?

Chandrasekhar:

That is what it was like in those days. During my years at Yerkes (by which I would mean essentially the late thirties and forties, those 15 years, that's the period I would call my Yerkes period) it was a scientifically and socially integrated community. Struve, as the unquestioned leader of the group, producing stability and balance. All of us were interested in one thing: to do the best work we are able to. And certainly in the late forties, after the war, we had some of the very best students we have had — as I told you, the three or four names already, and besides there were Ann Underhill, Nancy Roman, Frank Edmonds, Marshall Wrubel and then we had visitors like Stromgren and Minnaert and Oort. We never felt we were an isolated community, working in a vacuum. That feeling came to me after the early fifties, when I found I had to come to the campus every week.

Weart:

After the decline had set in. I see. Back in this very vital period, was there much discussion of things

outside of astronomy? Was there much talk about biology, politics, philosophy?

Chandrasekhar:

Very little.

Weart:

What about physics? Did people discuss new physics?

Chandrasekhar:

Well, of course, Fermi was on the campus, and his influence began to be felt. He came to Yerkes once or twice and I became friends with him, and I used to come to the campus to talk to him.

Weart:

About the contact with physics, I wanted to ask you some questions about his remarkable 4th Annual Conference on Theoretical Physics at George Washington Univ. in 1938 that you went down to. Do you know, for example, how that was organized?

Chandrasekhar:

That was Edward Teller's and Gamow's idea. They were at the University of Washington and they were interested in astro-physics at that time, and they arranged the conference. It was a marvelous conference. Rosseland was there. In fact, have a picture of the group which assembled there, somewhere.

Weart:

Do you recall, what was the character of this conference? It must have been one of the first times, I suppose, that these people got together, these different kinds of people.

Chandrasekhar:

First of all, there was a talk by Gamow on the nuclear reactions and Weicker cycles, based on Helium-5. Then, I remember, I gave a talk on the theory of white dwarfs, and Strömgren gave a talk on hydrogen abundances and the H-R diagram. Then Bethe got very much interested in the nuclear aspects, and Bethe one morning said that he had thought about Weizsäcker's cycles and that he had

alternative ideas, and he gave a talk right at the meeting on his carbon cycle.

Weart:

I see — was that the carbon cycle then already?

Chandrasekhar:

That's right, he gave the carbon cycle, exactly as he formulated it. And later he wrote the paper, and I read it. He gave me preprints of it and so on.

Weart:

I see. Because in that thing you published with Gamow and someone on the conference,* you don't mention the carbon cycle specifically. You simply talk about the idea of helium, and two protons getting together, and the possibility of the proton-proton cycle — * "The Problem of Stellar Energy," with G. Gamow and M. Tuve, NATURE 141 (1938), 982

Chandrasekhar:

that was also mentioned by Bethe, you see.

Weart:

Oh, that was Bethe who mentioned the proton-proton cycle?

Chandrasekhar:

Critchfield was there, and then of course, there was the question of the Gamow-Teller selection rules, or the alternative selection rules. Bethe proposed that hypothesis, then.

Weart:

— and the possibility of resonance levels was brought up — even something of the idea of shell burning —

Chandrasekhar:

Yes.

Weart:

Were some of these ideas new to you, this nuclear physics? Had you had much contact with nuclear physics before?

Chandrasekhar:

It wasn't new to me the sense that I had not read about these things. In fact, I gave at Yerkes that year a course on nuclear physics, based upon Rasetti's book which had just come out. I tried to keep abreast of what was going on at that time.

Weart:

What I'm trying to get at is whether this conference had a different character, in that the physicists were listening with great interest to the astrophysicists?

Chandrasekhar:

The only physicists who were there, physicists who were in some sense committed to the subject were — Teller and Gamow and Bethe, who was a great friend of theirs. Bethe was of course an expert in nuclear physics, and could pass judgement on nuclear physics, and of course you know, his ability to grasp things is so large that once he felt there was an area for nuclear physics there, he just bull-dozed in. And I think among the other physicists, there was Gregory Breit among the group, I remember, and Inglis was there, and Tuve. I think that was about all.

Among astronomers, there was Rosseland and my student Mario Schönberg was there.

Weart:

I think you give a list in your article, in fact.

Weart:

It's extremely interesting. We're still talking about the thirties, and I guess the next thing to ask is about your INTRODUCTION TO THE STUDY OF STELLAR STRUCTURE,* and how you came to it. The book itself is in a way self-explanatory. I was interested that you said in a way you came upon it from the white dwarfs, that you wanted to sort of do the white dwarfs and set them down. Were there other things, other reasons also? * University of Chicago Press, 1939

Chandrasekhar:

No, the fundamental reason was essentially that I felt that my future in science depended upon going into other areas, and that there was no point in my harping on topic and I simply decided that I would write a book putting all my ideas together, and extricate myself from the subject in a gradual way.

Weart:

Did you work on this over a long period?

Chandrasekhar:

No, I wrote the whole book in less than a year. I wrote it almost entirely in '38. I started in spring and finished it by the end of '38.

Weart:

I see, and you simply submitted it to the press.

Chandrasekhar:

Yes.

Weart:

It's very straightforward. It seems like we ought to say something more about such an important book, and yet I suppose it simply stands there. Did you have any feeling about it at the time it came out?

Chandrasekhar:

I was pleased with the way the book came out, but I don't remember any special exhilaration.

Weart:

What sort of reaction did you get from your colleagues, to this sort of a book?

Chandrasekhar:

Not very much I should say, at that time.

Weart:

Also, about this period — I don't know whether you're the right person to ask, but McDonald was founded and so forth. Were you —

Chandrasekhar:

I was present at the dedication of it.

Weart:

Were you familiar with all that went on?

Chandrasekhar:

Not really, no.

Weart:

We have the director's files, letters and so forth, on that. Well, let's see, we have to wrap up the thirties.

One thing that I wanted to remark, I notice a general absence of interest in general relativity and cosmology, all sorts of cosmology. Do you recall whether you had any strong feelings about general relativity theory and cosmology? Did you feel it was a field in which much progress could be made at that time?

Chandrasekhar:

Actually, I had made up my mind at that time general relativity and cosmology are the two areas in which I did not want to write. Because I felt that the involvement of Eddington and Milne, particularly, in general relativity essentially ended their careers. After all, Eddington started working on these cosmological things in 1928, and he kept on it till 1944, and I felt it was all wasted. Similarly Milne started his kinematical relativity in 1931, when he was in his late thirties, and he kept on for the next twenty years, and it all evaporated. I told myself that I did not want to go into that region; so I just wanted to settle on safe grounds. I was to return to relativity in the early sixties, and I remember that in 1961, when I had finished my book on stability, I wanted to change my area of interest and I was wondering

where I should turn, and I thought I would go into relativity. But I was really skeptical whether I should or not. I remember talking to Gregor Wentzel, who was a great friend of mine, and asked him what he thought of it, and he said, "Well, Chandra, if you want to work on relativity, why don't you go ahead and work on it?" I said, "Well, maybe I won't get anything very much." He said, "They can't fire you." So I gradually got into relativity only in 1961, '62, but even that after very considerable hesitation. Retrospectively, I think I did the right thing, both in not getting into relativity before, and in getting into relativity at that time. It is very difficult to rewrite one's life, but I think I'm able to do relativity at the present time, in spite of so many absolutely first-rate people working also [in it], largely because I have a background which enables me to do work in relativity of a kind which other people won't be doing. Therefore, coming at the stage I did, it was probably the right thing for me. But on the other hand, I doubt very much whether to have worked on relativity at an earlier time would have been as profitable.

Weart:

Did other people feel the same way, do you suppose, about general relativity at that time?

Chandrasekhar:

I don't know what other people felt. But I do know that it's true, they did not have very much respect for people who worked in cosmology.

Weart:

What about even extragalactic problems and so forth?

Chandrasekhar:

We were not doing any extragalactic problems at all. Everyone thought extragalactic astronomy consisted of one thing only, namely, the expanding universe. And Hubble was doing that, with the telescope that was available, and there was very little to do outside; that was what people thought.

Weart:

Was this Struve's idea or was it generally accepted?

Chandrasekhar:

I think it was probably the general feeling.
Extragalactic astronomy, as a part of astrophysics,
was a much later development in the sixties.

Weart:

It was the feeling one couldn't compete with Mt.
Wilson and Lick?

Chandrasekhar:

Yes. On the observational side certainly.

Weart:

Which is reasonable, I suppose. It's hard to get a
picture of the general field at the time. You
mentioned in the 1972 Halley Lecture* that the
staple of astronomy is the study of stellar energies.
You mentioned that earlier, the question of where
the energy comes from. Were you interested in
evolution? I don't see it too much in your papers, and
yet I get a -feeling from what you're saying that you
were interested in the problem of stellar evolution?

Chandrasekhar:

I was interested in it, but of course, due to historical circumstances, I gradually got out of the area. But on the other hand, the subject became the center in the forties. Martin Schwarzschild started writing on it, and all these evolution calculations started going on. If one looks at the astronomical literature, and certainly from the vantage point of the editor of the APJ, the one thing everybody was doing was drawing zero-age main sequences, and how to interpret the populations, how to interpret element production. Certainly the characteristic of astronomical development after '46, is one in which nucleosynthesis evolution of stars, was both the center of theoretical work, and the center of observational work. But at that time, I had dissociated myself from that line of development and was doing other things.

Weart:

I wonder how in general you kept up with what was going on? Of course you had a lot of conversation around the department and so forth, but I wonder also, did you get a lot of information about the

general state of what was going on from meetings, from letters?

Chandrasekhar:

No, as an editor.

Weart:

I'm thinking now again before that, before '46.

Chandrasekhar:

Then before that I was going to meetings. I used to be rather regular in attending Astronomical Society meetings.

Weart:

Were these important in getting to know what was happening, or did you already get it just by being at Yerkes?

Chandrasekhar:

No, half by being at Yerkes and half by going out. Of course there were people like Oort and Stromgren with us, - both extremely knowledged.

Weart:

What journals did you read regularly during the forties, let's say,? APJ of course.

Chandrasekhar:

APJ, MONTHLY NOTICES, ZETTSCHRIFT FUR ASTROPHYSIK. I was getting interested in hydrodynamics, so I was looking very much into the PROCEEDINGS of the Royal Society, which published much of the literature there. * "The increasing role of general relativity in astronomy," OBSERVATORY 92 (1972), 160-174.

Weart:

I see. OK, now we should talk about where you went next. Of course, all this period you were producing many papers on many subjects. I'm just trying to guess what the high points were. I guess the next high point is PRINCIPLES OF STELLAR DYNAMICS.* And your whole work on relaxation times. How did you happen to pick this problem next?

Chandrasekhar:

Well, it all started by my having to give some courses at Yerkes on stellar dynamics. I read the books which were available at that time, Smart's STELLAR DYNAMICS in particular, and it seemed to me that the whole subject was in a terrible mess; the simple problem of relaxation hadn't been worked on properly. When I went into a new subject like stellar dynamics or stellar atmospheres, I tried to study what other people had written. And I asked myself, "does it satisfy me?" And I found that in stellar dynamics at that time, one of the major problems was the problem of relaxation through two-body encounters. Nobody had done anything very rigorous about it, so I just said to myself, well, I ought to be able to do that.

Weart:

Did it strike you as a serious problem because of the problem of getting the age of the universe and so forth?

Chandrasekhar:

It came later.

Weart:

That came later?

Chandrasekhar:

Yes. I realized the implications of it as soon as I started on the work.

Weart:

I see, it was after you had started working on it. That's interesting. I almost get the picture, from this and from other things you've told me before, that you studied a great many things, particularly in connection with your teaching and so forth —

Chandrasekhar:

Yes.

Weart:

And then when you found a field — all through your career, when you found a field which was in mess, then you would go in and straighten it up. Is that a fair — ?

Chandrasekhar:

That's the way I went about it. Except in relativity.

Weart:

Where there's more of a continuing — ?

Chandrasekhar:

— well, in relativity, you see, I went in as an outsider, with diffidence. * Univ. of Chicago Press, (1942.)

Weart:

This is in the sixties you're talking about?

Chandrasekhar:

Yes. I went in with diffidence. And of course in relativity at the present time, there are enormous numbers of extremely competent people working. But the situation was not that in the other areas.

Weart:

They were neglected areas.

Chandrasekhar:

Neglected areas in which the people who had done work before, up to a point, had made a mess of it.

Weart:

Or just hadn't gotten very far.

Chandrasekhar:

Yes.

Weart:

And hadn't done it.

Chandrasekhar:

Yes. For example, I took radiative transfer, and I asked myself, "How do you write down the transfer equations of polarized light?" There was nothing on that. Nothing

Weart:

— nothing had been done?

Chandrasekhar:

Nothing had been done, so one starts right from the beginning. So in many ways, getting into general

relativity as late as I did was a most risky thing for somebody like me to have done.

Weart:

Yes, I understand.

Chandrasekhar:

From my earlier background.

Weart:

Though of course you had the mathematical background, or the theoretical background.

Chandrasekhar:

Yes. But apart from that, I had no reason to believe that I could make success of it, because I didn't even know what problems to work on. Whereas in other areas, you see, I started reading the subject, I found it in a terrible mess, and thought, "My God, I ought to put this straight," you see.

Weart:

In these other fields you didn't really have competition.

Chandrasekhar:

No.

Weart:

That's very interesting. I hadn't seen that before. That's true, when you were working on these books and so forth, there wasn't anybody else who was working on that thing at the time. Or likely to be. A little technical question on the relaxation times work — a technical sociological question, I should say — in the 1940 paper,* you thank "Mrs. T. Belland, who carried out the entire numerical work *"The dynamics of stellar systems. IX-XIV." APJ. 92 (1940), 441-642. connected with the tables." I'm interested in this because one knows that some time around in this period, numerical methods and powered machines and so forth began to become important. I'm curious about you whole introduction to numerical methods and machines and so forth — at what point did that come in?

Chandrasekhar:

Of course, you know, in 1935 when I did my white dwarf work I had to integrate all the white dwarf functions with a hand computer.

Weart:

Turning the crank, I suppose?

Chandrasekhar:

Yes. And I realized that in order to complete a theoretical problem, one had to have numbers. I was fortunate that at this time, Mrs. Belland was there, and Struve did not have any work for her, and he asked me, "Could you use her?" I said, "Well, I have this problem to work numerically," and so it started that way.

Weart:

She used a hand calculator.

Chandrasekhar:

Yes.

Weart:

Were there people around Yerkes to do calculations?

Chandrasekhar:

There were people; Struve had an assistant who reduced all his spectra, and van Biesbroeck was there with his comet orbits to be computed. So there were computers [i.e. computing assistants] in the observatory, yes.

Weart:

This was just the first time that you had had the use of one.

Chandrasekhar:

Yes.

Weart:

After that, did you retain one?

Chandrasekhar:

Gradually I started out. After the war, I had Mrs. Francis Breen who worked with me for some time. She did a lot of calculations for me on H minus. And of course, in late '49 Donna Elbert started working with me, and she is still with me here.

Weart:

It wasn't till after the war that you started to use punched cards and that sort of thing?

Chandrasekhar:

I've used very little of it, really.

Weart:

Still haven't used much of it. That's true, I see. Now, to get back more specifically to stellar dynamics and relaxation times. At what point was it that you began to get interested in the age problem? Almost immediately after you started working on it?

Chandrasekhar:

Yes. Because very soon I started working on the related problems of galactic clusters and the escape of stars from them. That immediately impinged on the age of the universe, and so I got into it. And Kuiper was there, who was very much interested in the age problem.

Weart:

It seems to me that the age problem is a problem that runs all through the thirties, this problem of reconciling things with the radioactive ages?

Chandrasekhar:

Yes. There was a part of that in it .

Weart:

It wasn't till '43 that you had a paper that raised the relaxation times enough so that it was fully consistent with the radioactive rates?

Chandrasekhar:

For the clusters, yes.

Weart:

Then also at the same time, this is work done in '41, there's your work with Louis Henrich. On the relative abundances of the elements and their isotopes.*

Chandrasekhar:

Yes.

Weart:

How did you get into that problem, from Weizsäcker, or?

Chandrasekhar:

Weizsäcker, yes. Of course, I was trying to follow the nuclear-physics developments at that time, and the Bethe cycle was one side, the other side was the origin of the elements. So I felt that there was something which one could do, namely redo some of the calculations the people had done at an earlier time, and concentrating on the isotopes in determining the age, rather than on the whole periodic sequence.

Weart:

This is one of your more cited papers, by the way, from your earlier period. People are still citing this. I wonder, how did you feel when the results came out and they were quite reasonable?

Chandrasekhar:

I was really very pleased. I remember, I gave it at the 50th anniversary celebration at Chicago. I had to give a talk in a session which the first one was given by Russell, I had to give the second, and the last was by Lawrence. It was a symposium here, in connection with the 50th anniversary celebration of Chicago in '41; and I wanted to give a paper which would in some sense be worthy of the occasion. So I worked on this particular problem the previous summer.

Weart:

If you can recollect, how did you feel about the Big Bang as an idea? Were you convinced of it through this period? * APJ. 95 (1942), 288-98.

Chandrasekhar:

I've always been on the side of the Big Bang all the time. Somehow it seemed to be the simplest solution. Though the present attitude is to go back to the closed universe.

Weart:

Back to the — ?

Chandrasekhar:

I mean, starting with the Big Bang, but ending also with a Big Bang.

Weart:

Well, that idea was certainly around in the thirties also.

Chandrasekhar:

Yes, but not very much in vogue. At least, not to the extent I knew. Eddington's famous remark, "I'm an evolutionist not a repetitionist."

Weart:

How did you feel about that?

Chandrasekhar:

I was not committed to it in a particular sense in my work, but I thought that the simplest explanation was in terms of an initial Big Bang.

Weart:

— and then of course, your own paper must have —

Chandrasekhar:

Yes.

Weart:

But perhaps you didn't need the confirmation?

Chandrasekhar:

No, I thought it fitted into that picture. But I didn't draw any overly dogmatic conclusions from it.

Weart:

Now, a question I suppose I'm obligated to ask, for the record. Many people have of course compared the oscillating cycle universe with Hindu cosmology and so forth. I wonder whether this ever had any effect on you, or whether you ever thought about these things?

Chandrasekhar:

No. In fact, I'm against reading our thoughts in ancient writing. If you believe certain things now,

and the English sentences which incorporate that belief happen to coincide with some of the sentences from the old texts, it doesn't mean that the thoughts behind it are the same. If you say certain things now, you say them for certain reasons, and those reasons cannot obviously have been in the minds of the people who wrote the same sentences at the earlier time. I don't go along with people who try to find confirmation of their ideas in the philosophy of the Koran, of the Bible, or any other ancient text.

Weart:

OK, fine. Getting back to the paper specifically, we haven't talked much about your work in collaboration. I wonder for example, in this paper as an example, what was the role of Henrich?

Chandrasekhar:

He was essentially a person with whom I talked about the problem, and I said, "Well, there's a lot of calculations to be done, let us do them together." The collaboration was not on a level of scientific equality, and it was in later years, particularly during the sixties, and seventies when some of my students

have supplied original ideas, and corrected me in some of my work.

Weart:

How did these collaborations work in general? Is there any general rule for how you work in collaboration with somebody?

Chandrasekhar:

Some students come along - particularly during the sixties - most of the students came from the physics department, and many of them were very good.

When I find after a while that I am able to communicate with them and we have compatible ideas, then we start working together. I tell them, "I have these problems; would you like to work with me, because there's a lot of work involved, and I do not know very much about the subject, I have to learn an awful lot, and we'll study it together." For example my last collaborator, John Friedman, was absolutely first rate. We started, and I was going to England, he came along with me to England. He integrated his work with mine. He was very good. I made several mistakes on the way, he corrected them. So it was on a level of scientific equality. That

was the way my collaboration with my students developed. But in fact the forties and early fifties, it was essentially one of my having a fairly well-founded idea, which I felt I didn't have the time to work through completely —

Weart:

In terms of working through the mathematics?

Chandrasekhar:

Or making some particular calculations, checking them, numerical work, these sorts of things — and asking them to go along with me on that.

Weart:

So there was a pattern at that time where you set up the framework, and then they would fill it in.

Chandrasekhar:

Yes.

Weart:

But now the pattern is somewhat different? Now they sort of come in at the initial — ?

Chandrasekhar:

They come in at the early stages. I haven't had a student for the last three or four years.

Weart:

But over the past 20 years, you would say.

Chandrasekhar:

Most of my collaborators, starting in the middle fifties, come in at the beginning on a subject I'm learning, and we learn together, we discuss it together, and mistakes are made and corrected.

Weart:

So they will work out some of the equations, you will work out some of them.

Chandrasekhar:

Yes.

Weart:

There's no clear division of labor.

Chandrasekhar:

That's right.

Weart:

I see. Now, here's another collaborative paper in the same year. This is Schönberg and Chandrasekhar*, I think there is a lot going on in the paper. To me the most interesting thing is that, I believe this is the first place where one sees quite clearly that stars evolve into red giants.

Chandrasekhar:

That's right.

Weart:

I wondered how you came to this paper, also.

Chandrasekhar:

Well, it all came this way. You see, one of the things which people talked about in those days, for example on which Gamow was writing in this — Gamow took the carbon cycle, and assumed that evolution was through a series of homogeneously contracting stars.

Weart:

Right.

Chandrasekhar:

But it didn't seem that could r ght. On the one hand, one assumed that there's a convective core. But the convection takes place only inside the core. And I knew that convection cannot come [bring material] outside. So the hydrogen must be exhausted in the center first.

Weart:

How did you know it couldn't come out from the convection?

Chandrasekhar:

Well, because there were all these arguments of Eddington, Sweet currents and so on. I knew sufficiently of the earlier work to know that it's matter in the convective core will be mixed. And if the hydrogen in the convective core is exhausted well, it must develop into isothermal core. And I felt the isothermal cores couldn't expand for ever and can grow only to certain maximum size. What will

happen then? I had done a similar work earlier. You know, Gamow had some idea about lithium burning, and coming right out to the outside. But I pointed out that in the early stages, the lithium can't be burned beyond a certain point. Then the question arose, what happens to the main constituents? And Mario Schönberg from Brazil was visiting me, and I said, "Let us do these calculations together." At that stage, we were doing hand calculations. There was a lot of work to be done, and he was very much interested in these problems, because he had been with Gamow earlier. So we worked together on that, and we were very surprised at what came out. In a way, you know, I sort of regret that I didn't pursue this matter beyond my work with Mario Schönberg. *APJ. 96 (1942), 161-72. Once we came to this maximum mass, you see, I realized that one ought to continue the integrations forward in a secular way.

Weart:

To revolve the star.

Chandrasekhar:

Yes. But I was then in the middle of my work with John von Neumann, and the radiative-transfer work

was progressing. I suppose it was a missed opportunity.

Weart:

But in fact, although I suppose this paper isn't too widely known, it is in fact the first paper to come out in this way. And I'm just wondering, when did you first begin to think — you say you were surprised, but when did you begin to think that this was in fact where red giants came from?

Chandrasekhar:

Well, I would say that I probably had the idea in '41, maybe.

Weart:

Did it come strictly out of doing the calculations?

Chandrasekhar:

No. Gamow was there doing of these things, you know, in Washington. He was publishing a lot of papers.

Weart:

Oh, you saw his published papers.

Chandrasekhar:

Yes. And also I corresponded with him, and I knew him quite well. You know, he was writing extremely speculative things, of how the variable stars come from lithium burning, beryllium burning, and I knew that all of that was wrong. So I began to think, and it seemed to me that the direction evolution has to go was along the lines Schoenberg and I had found. It came in, by and large through stimulation by work that Gamow was doing at that time, and my belief that the way he was going about it was not right, and that one had to go in an alternative way. Even though, when I wrote the book, I thought I was done with stellar structure, I kept on thinking about there problem. I had to essentially force myself to relinquish the subject, at a time when my mind was extremely active along these lines. I hated to do it but I had to make a choice.

Weart:

But then you were drawn back to it, when you saw this particular problem.

Chandrasekhar:

Yes.

Weart:

Before this paper or before 1941, what were your opinions of red giants, of the whole direction of evolution?

Chandrasekhar:

Let me put it this way. During those years, after 1939, I was always in stellar structure with a mild protest. I was seeing what was going on, and I was being consulted by people like Bethe and Gamow, and so I was constantly made to think. And so, I felt that some of the things that Gamow was writing were so completely wrong — and since nobody else was working at that time, because of the war, I felt that I had to go in and do something. I really got interested, seriously interested in it, when Mario Schönberg and I were working on the subject, and I wanted very badly to go on. But again when Schönberg left —

Weart:

But before this period, did you have in your head some sort of mental picture of the H-R diagram, with some sort of evolutionary track — ?

Chandrasekhar:

it was emerging at that time. It was not well formed.

Weart:

You didn't have a definite idea that stars start as red giants and go in this direction or whatever?

Chandrasekhar:

That was my idea, yes. I mean, I definitely had the idea that once a star moves away from the Main Sequence, it might go into the red giant branch, you see. And I said, "One ought to follow it in a secular way, looking at the evolution." That is something which I wanted to explore. I felt that was the direction to go. But I had no absolute conviction that the answer would come out that way.

Weart:

Did you discuss these things with people around Yerkes? Was there much discussion of what sort of tracks one might have in the H-R diagram?

Chandrasekhar:

No, I didn't.

Weart:

People were not much interested?

Chandrasekhar:

I talked with my students. I talked with Henrich, for example, quite a bit, and I talked a little to Martin Schwarzschild.

Weart:

He must have been very interested in all these things.

Chandrasekhar:

Yes. But of course, you see, we weretogether in the Aberdeen Proving Grounds during the war, and then

he went to Italy, and by the time he came back at the end of the war, I had left the area, pretty nearly.

Weart:

Well, maybe this would be a good place to stop. Tomorrow morning, we'll talk about the Aberdeen Proving Grounds, and then — when things get too recent, we have to be careful what we talk about —

Chandrasekhar:

Yes.

Weart:

But I certainly want to talk about things that happened at Yerkes, about your editorship of APJ — about the whole history of APJ — and of course, if we have time, also about your postwar work.

Interview Session – 2

Weart:

The first thing I want to ask you is whether you think there is something we didn't cover for the period up to the start of the war? Have you had any thoughts about something we may have missed?

Chandrasekhar:

I don't think so. I mean, I can't remember too well.

Weart:

Well, good. Let's go forward, then. You will get a chance to read the transcript and see then whether there seems to be something missing. Let's start with the war, then. You worked as a consultant at Aberdeen Proving Grounds.

Chandrasekhar:

Yes.

Weart:

I really don't know how you got into that, how the war first affected you, what you did there.

Chandrasekhar:

Well, naturally, with all young men of the time, I felt deeply committed to the effect of the war on civilization, and I wanted to participate in the war effort. I was not a citizen at that time, and consequently I had special clearance problems.

Weart:

Had you applied for first papers?

Chandrasekhar:

No. The condition under which we came at that time was that we could not have become citizens, because the Immigration Act did not allow people of Asian origin to become citizens. I was a permanent resident. It was only the Immigration Act passed after the war which made it possible for us to become citizens. But we took out our citizenship papers only in 1953. Until that time, we were permanent residents, and we could not have become citizens much earlier. (We might have become a year or two earlier, but we had to wait for the new immigration laws.) But as a British citizen, as I was at that time, I got clearance to work. Largely under

the influence of John von Neumann, I joined the Aberdeen Proving Grounds.

Weart:

Perhaps this would be a good time for me to ask you about your relationship with von Neumann, how it got started?

Chandrasekhar:

Well, von Neumann was in Cambridge in 1934-1935. The year in which I had my controversy with Eddington, and Neumann was one of the people who privately supported me against Eddington. Of course all these people who supported me never came out publicly. It was all private.

Weart:

In private means that they would tell you they supported you.

Chandrasekhar:

Yes. But they wouldn't want to publish anything on that. Anyway, I got to know Neumann rather well. I was a fellow at Trinity at that time, Neumann used to visit me in my rooms in Trinity quite frequently. I

think he was rather lonely in those days, so he would quite often come up to my rooms in the college and sit down and work in my rooms, and so I got to know him rather well.

Weart:

He would use your rooms for working.

Chandrasekhar:

Yes. We used to go out for walks. So I got to know him rather well at that time.

Weart:

Did you discuss your work together?

Chandrasekhar:

Yes. Indeed, at that time we started to work on some problems in relativistic gas spheres; it didn't go very far. I do remember our discussions of that year, and I did some work and published a paper in the late early seventies, on precisely the problem which Neumann and I discussed in 1934 — the problem of isothermal gas spheres in general relativity. In a way, it shows Neumann's great insight. He said, "If objects are going to collapse, then they must

collapse to smaller dimensions. We ought to look at it in the frame work of general relativity." We looked at it, and this was, several years before Oppenheimer and Volkoff did their work. Indeed, I am sure I have somewhere in my files Neumann's and my notes on our work with relativistic equations of equilibrium.

Weart:

You were on the track, in fact.

Chandrasekhar:

We were in the right direction. And in this instance I must say that it was Neumann who took the initiative.

Weart:

What did you think of him at the time?

Chandrasekhar:

He was incredible - the enormous perception that he had. For me, ever since, a standard of comparison has always been von Neumann. And if I say, "He reminds me of von Neumann," that's about the best compliment I can give anyone. Bob Geroch in our

department here is someone who does remind me of von Neumann.

Weart:

Many mathematicians have suffered in fact by comparing themselves with von Neumann.

Chandrasekhar:

Yes. He was incredibly perceptive. I knew him very well from 1934 till his death. For example, when we came to this country in 1937, I used to go to Princeton and see him regardless. And it was on this account that we later collaborated in a series of papers in the early forties. In fact, my first visit to Princeton, in the fall of 1941, was at Neumann's instigation.

Weart:

You went to work on astrophysical problems?

Chandrasekhar:

Astrophysical problems. Yes I just told Johnny that I was rather tired of having been at Yerkes for several year that, I would like some fresh air, and could he make anything possible for me. He said, "Why don't

you come to Princeton and spend some time with me?" I said, "Fine." So I went there. He also arranged that I had a part-time appointment at the observatory also and this was the time I got to know Russell very well.

Weart:

I see, in the early forties.

Chandrasekhar:

And during those three months, I used to see Henry Norris Russell quite a lot, you see.

Weart:

Was he interested in these problems at all?

Chandrasekhar:

Not in the statistical problems I was working with von Neumann; but we used to talk about stellar structure. In those days, I wanted to broaden my interest in astronomy as much as possible in all areas, and the chance to talk to Russell was a chance to learn astronomy.

Weart:

I see, and he was receptive to having this kind of interchange?

Chandrasekhar:

Oh yes. In fact, I felt in those years Russell was rather lonely. Sven Rosseland had just come from Oslo and joined the Princeton faculty. Rosseland is a very, very quiet person. He rarely talks to people, at least he did not during the time I knew him. So Russell was rather lonely. Whenever I used to go to the observatory on Prospect Avenue, Russell would see me, invite me to his office. And he will talk to me sometimes for hours, about his work on eclipsing binaries, and also ask me about my work and so on. He was an enormously receptive person, and at the same time, enormously communicative.

Weart:

About astronomical matters.

Chandrasekhar:

Astronomical matters, and also about his own life. He used to tell me about his grandparents and

parents, who always used to live on Alexander St. in the house that he owned. He told a marvelous story of how once a burglar came into his house, and his mother, who was very young at that time, saw the burglar and asked him, "Where did you come from?" And the man who was so frightened by the coolness of this lady that he fled! So Russell was very communicative. I used to have long talks with him, and he used to tell about, Princeton's early days. I don't think Princeton appreciated him, because Russell was kept in a junior position, at a time when Jeans was a professor. Somehow, that colored Russell's own view of Jeans, At least that's my impression.

Weart:

Did it color his views of Princeton? Did he still remember that?

Chandrasekhar:

Well, you know, he was kept in a low position for a very long time — I think he was recognized far more in Europe than in the U.S. Because his department was small; other people in other parts of the university did not know his work. He was working

in eclipsing binaries and light curves and getting mass ratios. All this would appear to a physicist as rudimentary geometrical and elemental. He did not realize the astronomical implications. The Russell Diagram came out of that.

Weart:

Was this till at all in the forties? Or are you referring mostly to the earlier period?

Chandrasekhar:

Well, of course, by 1920 he had been recognized abroad, and he had been given the Gold Medal of the RAS. He had received the Bruce Medal, and of course his position in astronomy became very high. Later of course he did spectroscopy, in the twenties, at a time when spectroscopy was in the limelight of physics, and came out with the Russell-Saunders coupling. By the twenties, he was recognized in astronomy amply, and in physics as well as one of the leading figures. People like Shenstone, Meggers, Kees, all these men came under the influence of Russell. Russell was the dominating person. I remember a marvelous conversation between Russell and von Neumann. I was walking together

with both of them and they were going to a committee meeting. (I was not in the committee.) Von Neumann asked Russell, "How does it happen, Professor Russell, that you have been at this university for so long a time and yet you are on so few committees?" Russell said, "There is one principle by which you can get off committees. When you are on a committee, no matter what the subject is, talk endlessly, prevent other people from talking. They won't put on another committee after that. Neumann told me later that in the committee on which he served with Russell, Russell had kept his word.

Weart:

In the astronomy department he was recognized as the leading figure?

Chandrasekhar:

Of course the astronomy department consisted of only 3 people, Dugan who was quite ill at that time, Steward — and Russell. Russell is known to say my book Russell, Dugan and Steward.

Weart:

And how were relations between the astronomers and the physicists at Princeton? You mentioned Russell talking with von Neumann and so forth. What about with the others?

Chandrasekhar:

I was too young to notice those things. But of course Russell was the great figure. I met Russell first in England in 1934 when he was visiting Cambridge as a guest of Eddington. Eddington asked me to join him for tea with Russell. I was at time in my first year of my fellowship at Trinity. So I got to know Russell at that time, rather briefly. And later of course in '36 I was his guest in his house. I really got to know him well in those three months in 1941 at Princeton.

Weart:

I've heard it said that Russell was a charming man and so forth, but that there was within him a core of, I don't know what, uncertainty perhaps, some feeling about himself which never showed very much to outsiders? Did you get any feeling — ?

Chandrasekhar:

Well, the only way I can answer is the following. When I knew Russell well, I was in my early thirties. In the fall of '41 I was just past 30. But my attitude to science at that time was always of someone very low looking at the very high. Therefore my vision of Russell was of an enormous big person in science, and that prevented me from comparing him to anyone else. On the other hand, thinking back, I remember his telling me at one time very simply, during a conversation in 1941 — "I had the Pauli Principle right in my hands. I let it go." [Holding up two cupped hands, then letting them drop.]

Weart:

And he held his hands up like that?

Chandrasekhar:

Yes. And he looked very sad. Similarly when he talked about the early years, he seemed to be a little bitter. I can't explain his rather transparent dislike of Jeans, [except] as in some way resulting from the enormous position Jeans had in Princeton, while he

[Russell] was the assistant of somebody or other at the observatory.

Weart:

Like Milne and Eddington.

Chandrasekhar:

No that comparison is not correct. Milne had a big following of his own I mean, he was distinguished in his own right, and Milne of course looked up to Eddington enormously. It was, in fact, his great admiration for Eddington which reacted unfavorably in a psychological way. When they fell out scientifically. But Jeans and Russell were contemporaries. They were of the same age, whereas Milne was at least 10 to 15 years younger than Eddington.

Weart:

I see.

Chandrasekhar:

So the situation was slightly different.

Weart:

Well, maybe now we should get on to the Aberdeen Proving Grounds.

Chandrasekhar:

Well, at Aberdeen Proving Grounds, I got to work on ballistics problems related to shock waves, particularly to the Mach effect. There is some reference to my work in Courant's book on shock waves. I was interested in working on the theory of shock waves. Martin Schwarzschild was in the Army — and though in the Army, he was also working in Aberdeen Proving Grounds — and we were there together during that time.

Weart:

How long were you there?

Chandrasekhar:

I used to spend three weeks at Aberdeen Proving Grounds, come back to Chicago, lecture in Chicago to the students for three weeks, then go back to Aberdeen Proving Grounds for three weeks, and this I kept on for about 2 1/2 years. It was a very strenuous period.

Weart:

Yes, it must have been, going by train back and forth. How did you feel about doing this kind of work? It's a very immediate thing, whereas astrophysics is so long range with no immediate applications.

Chandrasekhar:

I simply felt that everybody was spending their time on the war effort, and I didn't see why I shouldn't) particularly as I was strongly in sympathy with the underlying motives for the war. Of course, during the Second World War, the whole atmosphere was quite different from what it was during the Vietnam War.

Weart:

oh yes, I'm not asking about that, just —

Chandrasekhar:

We were completely and totally committed to the war. Everybody was committed to the war. I, in common with everybody else, was committed to the war.

Weart:

How were the problems set that you worked on? Did you look around for a problem? Did a general come in and say, "We want you to work on this"?

Chandrasekhar:

No. I joined a group of people with Robert H. Kent, who was quite a well known ballistics man. He said that they were very much interested in reflection of shock waves, and how shock waves propagate, and he asked me if I would want to work on it. So I took up the subject, studied it, and worked on it along the lines directed. Then there were the problems of fragmentation; how will a bomb fragment, and what are the advantages and disadvantages of dropping bombs on the ground and above the ground; and what is the maximum effective height to drop bombs so that the fragments will be sprayed over the largest area. These are all problems which anyone trained can think about.

Weart:

Were you working more or less alone? Were you working with Schwarzschild?

Chandrasekhar:

I was working primarily with Martin Schwarzschild so long as he was in Aberdeen Proving Grounds, but very soon he was sent over to foreign service in Italy. Later I was working with Robert Sachs, who is now the director at the Argonne National Laboratory. He joined that group. L.H. Thomas, and Hans Levi of Berkeley belonged to the same group.

Weart:

Did this work have any effect at all on the astrophysical work that you were also carrying on? Was there any transfer of ideas?

Chandrasekhar:

Not at that time, but later on, because it was my first serious introduction to hydrodynamics. I learned hydrodynamics at that time, but it did not have any immediate effect on the work I was doing in astrophysics. But in the fifties, when I went into hydrodynamics proper, all the things that I had done before had overtones.

Weart:

Now, a couple of questions that I like to ask everybody. When did you first hear about the discovery of nuclear fission, in '39?

Chandrasekhar:

I heard about it almost as it happened; I mean, in the sense that, within a week or two after it was discovered, Sam Allison talked about it. In fact, I gave a whole series of lectures at Yerkes, on what was happening on nuclear fission in 1939.

Weart:

I see. You heard about it from Allison.

Chandrasekhar:

And then I read the papers as they came along. I was in England during the summer of '39 in connection with the Paris meeting on white dwarfs, and I talked to many of my friends there — Norman Feather, and Oliphant who was also there — all these were my friends from Cambridge from earlier times. So we talked a good deal. In fact, Niels Bohr visited Cambridge during the week I was there, and he

made one of his talks about nuclear fission and U235 was the 35 or 38.*

Weart:

I see. He came over and was in the United States at the time.

Chandrasekhar:

Yes. But he also talked at Cambridge when I was there for a month during this period.

Weart:

He must have been very excited about it. Did you feel that the people at Chicago, particularly at Yerkes, had any particular interest in it?

Chandrasekhar:

Not the people at Yerkes. But I was very much interested in it, and I remember I gave a colloquium on nuclear fission in the astronomy colloquia at Yerkes. Struve and others were very interested in that. I knew Sam Allison quite well, and Bethe, of course. Bethe came to Chicago and we used to talk about it. And during the war, I used to come to the campus rather regularly, and used to meet Wigner,

and of course in a way I knew in a very general way what all these people were working on.

Weart:

When did you first have some idea that atomic bombs could be built?

Chandrasekhar:

It was freely talked about in '39. At Cambridge somebody asked Niels Bohr, "Could you make a bomb out of it? And Bohr replied, "We have too many bombs already." The fact that more neutrons came out [from fission] suggested to everyone that one could use it. But beyond that simple idea, that it could be done, no one really pursued it, except those who were inside. * I.E. whether the fissionoffe isotope was uranium - 235 or 238.

Weart:

This was the possibility of buidling a bomb. I wonder, at what point — did you know before Hiroshima that a bomb was going to be built?

Chandrasekhar:

Actually, von Neumann tried to persuade me to join Los Alamos. And indeed went through all the clearances and things. But somehow in the last moment I decided to stay on at Aberdeen Proving Grounds.

Weart:

Why was that, I wonder?

Chandrasekhar:

Well, the whole idea of moving to Los Alamos seemed rather a big change for me. And even at Aberdeen Proving Grounds I used to encounter racial prejudice in many forms — in restaurants and places like that — and I was slightly afraid of driving down to the south.

Weart:

I can appreciate that. While you were here in Chicago you said Wigner, you must have seen Fermi, and so forth. Did they ever draw on your knowledge? You were working on stochastic

problems, they were working on stochastic problems.

Chandrasekhar:

Yes. In fact, I remember very well giving a colloquium on one of my problems — at that time on radiative transfer, which of course was used extensively in the Manhattan Project — and I had an enormous audience, including Wigner and Fermi and others, and everybody was surprised and I was too. But then I realized very soon that it must be in connection with what they were doing.

Weart:

Did they ever come to you and say, "Here is an equation, what do we do with this equation?"
Anything like that?

Chandrasekhar:

Not in a very direct way, but I do remember that Wigner was very much interested in my continuing work on radiative transfer, and he asked for reprints and preprints. But never directly asked me any question.

Weart:

Never directly posing you a particular problem or whatever.

Chandrasekhar:

Yes.

Weart:

Let me ask you also, because I like to ask everyone — what was your reaction when you heard about Hiroshima?

Chandrasekhar:

In a sense, it was not a surprise, because I knew this work was going along. On the whole, I was rather disappointed that it was dropped a second time. In fact, I remember being quite angry. I thought one was excusable, but the second did not seem to me necessary at all.

Weart:

Did it seem to you, as some people have maintained since, that it was racist?

Chandrasekhar:

I didn't think of it that way. But it did occur to me that if the war in Germany had not been over, the bomb probably would not have been dropped on Germany.

Weart:

Well, to get back to your astrophysical work, your scientific work during this period, this was during your three weeks at Chicago, you were doing astrophysical problems?

Chandrasekhar:

Yes.

Weart:

So somehow you maintained sort of compartments. When you were at Aberdeen, you worked on shock waves. Here, you worked on astrophysics.

Chandrasekhar:

Yes, That's what I used to do.

Weart:

Was this a conscious thing, you needed — ?

Chandrasekhar:

In a way, yes. I didn't want to leave my scientific work. In fact, you raise an important question. One of the fears I always had for very many years was whether I could continue generating scientific problems, nontrivial problems, for long periods of time. I sort of felt that if one gave up doing serious scientific research for a period, then one might not be able to get back to it. And so, in order to essentially protect myself against losing a grip on science, or somehow stopping the flow of ideas, I kept on.

Weart:

I see. Is this also why your work tends to overlap — even after you finish a book, there will be a certain momentum and you'll continue producing a few papers? While you're starting the next one?

Chandrasekhar:

That's right. I make an overlap. I gradually terminate one, while picking up something else.

Weart:

Till the momentum gets going.

Chandrasekhar:

Yes.

Weart:

I see. There are several things you did during this period. We should first talk about this article in *REVIEWS OF MODERN PHYSICS* on stochastic problems.* As you know, this is one of the most cited [of all scientific] articles. I wonder how you got into doing that?

Chandrasekhar:

The origin was as follows. I had already calculated the time of relaxation and problems of that kind, and I didn't quite like the arbitrariness in some of those calculation. One had a cut-off; further, one always said that the stellar encounters had a cumulative

effect, but one treated it as though the collisions were ones in which, after the collision, the two particles went off in directions entirely different from what they were before. It seemed to me that as one in Brownian Motion the right way to look at the problem. So I started learning the theory of Brownian Motion. * Vol. 15 (1943), 1-89.

Weart:

In order to approach the stellar problems.

Chandrasekhar:

Yes. [Tape # 1 (Side 2)]

Chandrasekhar:

I found that there was no really satisfactory book or account. So I went back to the original sources, particularly Smoluchowski's papers. I wrote for my own benefit a complete account of the subject. I happened to show it to von Neumann, whom I used to know quite well, during those days and he said, "Well, I've never seen such a clear account of this whole subject. You ought to publish it." I said, "Where can I publish it?" He said, "REVIEWS OF MODERN PHYSICS." I said, "Well, I don't know if

they will accept it if I send it." "But I will send it for you." And so von Neumann sent the paper to Buchta, and they accepted it. So my interest in the field of Brownian Motion was to use it as a basis for the theory of stellar encounters since I felt the theory ought to be modeled on the theory of Brownian Motion. It was in that connection that I worked out the theory of dynamical friction, and used the Fokker - Planck Equation. I believe I was the first to use the Fokker-Planck Equation for stellar encounters and work along those lines.

Weart:

In fact, the REVIEWS OF MODERN PHYSICS article is not just old material put in new form, but it actually contains new material.

Chandrasekhar:

Yes. Quite a lot of new things are there. But it was all new in the context of what other people had done.

Weart:

You were talking yesterday [off tape] about the way that lives of artists and so forth are divided into early, middle and late periods, and when I was

looking at this article with that in mind, it occurred to me that this almost could be said to be the start of your middle period. It appears as a step away from astrophysics towards connecting with physics. Did you have any feeling of that at the time, that you were moving towards physics?

Chandrasekhar:

Implicitly, yes. But the real change into physics came only in the late forties, because while I was doing all this work, on stellar encounters I got involved in radiative transfer and the problem of the negative ion of hydrogen, and these were, in many ways, very specifically astrophysical problems.

Weart:

How did you get involved in radiative transfer? I didn't ask it though it goes back to —

Chandrasekhar:

— the same period. Well, I was lecturing on stellar atmospheres, and was not at all satisfied with the existing treatments of radiative transfer. Problems of phase functions, problems of solving the equations systematically, trying to get exact solutions — mean,

all that, people hadn't done. So I started the sequence of papers, and almost at the time I started it, I read the paper by Wick in which he had used the method of discrete coordinates,* and I realized at once that that method can be used in a large scale way for solving all problems. So that went on. I have always said and felt that the five years in which I worked on radiative transfer [1944 - 49] is the happiest period of my scientific life. I started on it with no idea that one paper would lead to another, which would lead to another, which would lead to another and soon for some 24 papers — and the whole subject moved with its own momentum. Occasionally I had to come in and push it here or push it there. But the subject seemed to develop on its own. Particularly when the principles of invariance came. The paper by Ambartsunian** which I saw, seemed to me rather specialized; I could see that his one principle of invariance could be generalized into four principles of invariance, applicable to finite atmospheres, Ambartsunian's work was restricted to semi-infinite atmospheres, and used to solve all problems exactly. All this had a momentum of its own. Then suddenly I realized one had to put polarization in; the problems of characterizing polarized light — my rediscovery

of Stokes original paper, writing on Stokes parameters and calling them Stokes parameters for the first time —

Weart:

Oh, is that the first time they were called Stokes parameters?

Chandrasekhar:

Yes, there was no reference to Stokes' work prior to my work for some 50 years at least. I found Stoke's papers, and then called them the Stokes parameters. Then, of course, I finally wrote my book on radiative transfer.*** So even though my work on the theory of Brownian motion was a starting point towards physics, the fruition of that development was delayed for five or six years by my incursion of radiative transfer. And they were my happiest years.

Weart:

I see. You found yourself entering a new world.

Chandrasekhar:

Yes.

Weart:

And no one else was going into it.

Chandrasekhar:

Yes. And I still regard those five or six years as the happiest of my scientific life. * G. C. Wick, ZEITSCHRIFT FUR PHYSIK 120 (1943), 702 ** ASTRONOMICAL JOURNAL (Russian) 19 (1942), 1 DOKLADY (C.R. Acad. Sci. USSR) 38 (1943), 257 JOURNAL OF PHYSICS (Acad. Sci. USSR) 8 (1944), 65 *** RADIATIVE TRANSFER (Oxford: Clarendon Press, 1950)

Weart:

It was happy because the development proceeded so smoothly?

Chandrasekhar:

Well, that was one thing. The development was natural, and the subject fascinated me. It had a beauty of its own. Because all these principles of invariance, these nonlinear integral equations — the way they can be solved exactly; the fact that the polarization problem could be solved exactly, for the

first time. I mean, all that meant that I was solving problems for the first time, for which people hadn't even thought of formulating equations. That was one aspect. And the second, of course, was that I had arrived from a state in which I was always looking up. I felt for the first time that I was on my own, and that I was doing things without being intimidated by bigger people in front of me. Because the subject seemed to be carrying me on. I felt completely free, for the first time, scientifically. And also, I felt that my position in science up to a point was secure. I knew that my two books were becoming standard, and I'd been elected to the Royal Society at that time, and so there was a youthfull glow in my life which I have never recovered.

Weart:

Were there particular points at which you suddenly said, "Ah, now I have this"?

Chandrasekhar:

Well, if you look at those papers on radiative transfer, you will find a paper will conclude by saying, "These are the fundamental problems to

solve. And in, note added in proof; "These problems have since been solved," and so on.

Weart:

There wasn't any one particular point, it was a whole sequence?

Chandrasekhar:

A sequence.

Weart:

Solving and solving —

Chandrasekhar:

And solving. In between, I came into H minus. Of course, at that time, even Wildt had given up H minus, because Rupert the maximum was not at the place that he wanted.

Weart:

How did you get into H minus? I noticed you had two papers, one around '45, pointing out the problems in the earlier work, and then the next one solving them.* How did you come to the problems?

Chandrasekhar:

It was again the same. I was lecturing on the subject and everybody said that the H minus absorption wasn't adequate and on the other hand, I felt that the way cross sections had been derived was not satisfactory. So I said, "Let me try to do it better."

The key to the solution of H minus was the fact that I calculated the matrix elements using the accelerations operator as the momentum operator, rather than the dipole moment (which did not give it right). And in fact, Wigner played an important role. I remember talking to * APJ. 102 (1945), 223-31, 395-401; see also paper with F.H. Breen, APJ. 104 (1946), 430-45 Wigner here in Chicago at lunch.

"Well, Eugene, People always compute the cross section with a dipole moment. Why shouldn't one compute it with a momentum?" He said, "Of course you can." But I said, "Shouldn't the dipole moment give better formulae for things like H minus?"

Wigner said, Yes. I had come to Chicago that Wednesday for doing some job, but after talking to Eugene, without doing the job, I went back to Yerkes and did the entire calculation with the momentum and acceleration operators in that one week, and found that the maximum had shifted from

4500 to 9000 [Angstrans]. So I came back to Chicago the following Wednesday to show Wigner the calculations. It was a very exciting period, you see.

Weart:

Did you feel also that this was a period when your work on radiative transfer and H minus was of great interest to your colleagues at Yerkes?

Chandrasekhar:

Well, I'm afraid it never occurred to me. I was very much disappointed when later on I found that the work in fact was not appreciated by my colleagues. But during the time I was doing it, somehow I felt I was working in an area in which the subject pleased my taste. The mathematics was just exactly right for me. There were new types of mathematics, new types of integral equations. I was solving problems for the first time, which people had written about, Rayleigh had written about a hundred years before. And Stokes had used Stokes parameters, here I was using them for solving real problems. Then H minus came along. Rupert Wildt was a great friend of mine, and Rupert Wildt was terribly appreciative of

the work I was doing on H minus. Of course, the H minus work was immediately recognized.

Weart:

That must have had immediate effect.

Chandrasekhar:

But my radiative transfer work was not. On the other hand, in the middle fifties, the Rumford Medal was given to me for my work on transfer theory.

Weart:

Certainly since then people have recognized it. It must be one of your most used books. I suppose you know from your sales — RADIATIVE TRANSFER must be one of the most used.

Chandrasekhar:

Yes.

Weart:

But you say, at the time, the people at Yerkes were not terribly interested?

Chandrasekhar:

I didn't think they were. But on the other hand, I was surrounded by this glow of the beauty of my radiative-transfer work and the rest of the world didn't matter to me. In fact, in many ways that is the way one ought to do science — totally engrossed in what one is doing, interested in what comes out, in the enlargement of understanding it produces. And in a way, with pure joy. None of my later work or earlier work had those characteristics.

Weart:

Because of the nature of the field?

Chandrasekhar:

Well, when I was working on hydrodynamics in the fifties, it was pretty hard work.

Weart:

It's very messy.

Chandrasekhar:

I would not say that. Hard work, and terribly grinding work. Later when I went into relativity — again very hard work.

Weart:

Were you still doing the Aberdeen Proving Grounds work while you were doing this radiative transfer?

Chandrasekhar:

Yes.

Weart:

It must have been quite a jar — to drop it, in the middle of a problem, to go to Aberdeen.

Chandrasekhar:

Yes. Fortunately for the world, the war lasted only one more year (after I started radiative transfer).

Weart:

Another incident in the radiative transfer which, you've already mentioned is making allowance for polarization. How is it that you happened to think of

it, or perhaps more accurately, why do you think that nobody else had thought of it?

Chandrasekhar:

Well, people simply thought it was too difficult. I quoted in the RADIATIVE TRANSFER this remark of King's — [gets the book]

Weart:

This is on page 286, here.

Chandrasekhar:

"The complete solution of the problem, from this aspect, would require us to split up the incident radiation into two components, one of which is polarized in the principal plane, the other at right angles to it. The effect of self-illumination would lead to two simultaneous integral equations in three variables. The solution of it would be much too complicated to be useful." And then I add:

"However, it should be noted that for a complete description of the partially plane-polarized radiation field, it is not sufficient to consider only the intensities. A third parameter used is necessary to allow for varying the plane of polarization of the

radiation. Even so, we have found that it is not too difficult a matter to formulate the correct equations and solve them exactly." The point is, every problem I started to work, with my method, was soluble. I said, "Let me try harder and harder problems." I said, "Nobody has tried polarization, let me try polarization." And it worked! It was all these things in which one only had to have the audacity to ask the question, and the method answered it. The principal thing was to ask the question, and once you asked the question, the method was to solve it. There was no difficulty after that. It's a marvelous feeling, to do science in that way. You know, when I wrote the book, I wanted ever so much to go on beyond that. But I told myself, I don't want to spoil what I have there. Let other people do the spoiling. In other words, after having written the book I felt that there is a unit, representing a work which I have enjoyed most, and which is written exactly the way I want to write. I don't want to spoil it by my messing with it — if other people want to do it, it's their job. For me, that is it, and I didn't want to spoil it.

Weart:

I see.

Chandrasekhar:

In a way, it's like going to a marvelous dinner, and then saying, "I won't over-eat, because that would spoil the effect." It's very difficult for me to get enthusiastic about it now. In 1970, there was an international conference on radiative transfer, and I was asked to give the opening address. I wrote to the president, "I haven't talked or thought about this subject, literally not talked about it, for 15 years. I haven't followed the literature. I don't want to do it." But the President wrote back, "Well, we all use your book, we should like to have you come and give us a talk." So I agreed. The conference was in London. I had here a summer school in relativity, and I was working on relativity problems right up to Friday evening. On Sunday I was to take the plane, go to England on Monday, and the talk was scheduled for Tuesday morning. After Saturday morning, I started to think about what I was going to say. And I couldn't think of anything to say. Finally on Sunday morning, I called my collaborator and friend

Norman Liebowitz and said, "Why don't you come here and talk on the subject?" So I started talking, and since I had to talk, I talked about half an hour, I remember, and got into the mood of the subject. That was all the preparation. Then that afternoon I took the plane, arrived in Oxford late Monday afternoon, and Tuesday morning, I had to give the lecture*. * There is reference to this talk in the Proceedings of the Conference, published in The JOURNAL OF RADIATIVE TRANSFER I hadn't thought or prepared, except for that half an hour on Sunday. I started talking, and it was unbelievable — after the first five minutes, I could write all the relevant equations on the blackboard, talk about the problem with complete and total eloquence. Because I recaptured the whole spirit. I remember, at the end I said that, "I thank you for your patience with an Ancient Mariner." Everybody told me afterwards that they would not believe that I had not prepared it at all. I'm telling this because when I talk about that period, I'm always nostalgic about that period, because it was a time I was happy in science. Before that, there was unhappiness connected with Eddington, controversy, and a certain diffidence whether I could make the grade in science. Not that I

had enjoyed doing Sciences I felt that my earlier diffidence was unjustified.

Weart:

That the problem itself was the important thing.

Chandrasekhar:

The important thing. And that if you enjoy doing science, then that's enough, you see.

Weart:

And since then you haven't been as happy?

Chandrasekhar:

No. One wouldn't see it in my published work.

Weart:

It just hasn't been the same, because the problems haven't been the same?

Chandrasekhar:

In a small sense, I think my work on the Kerr metric over the past years has recaptured for me that old

spirit. But relativity is a very difficult subject, very hard.

Weart:

Yes, indeed. And you don't have it all to yourself, in the same sense that —

Chandrasekhar:

Yes. There are incredibly good people working in relativity.

Weart:

When you worked on radiative transfer, did you feel that you were working in a complete vacuum, as far as anybody else in the world goes?

Chandrasekhar:

I felt that it was absolutely fresh ground. Someone once told me "I want to ski on a mountain where nobody has skied before." Well, I don't ski, but in some sense, it looked as though I was going into a field where there were no footsteps there before. I was just going right along-at my own speed.

Weart:

I see. To get back to the polarization story, you mentioned yesterday over lunch about having it checked observationally. I wonder if you would repeat that story?

Chandrasekhar:

Well, that was a little later, after the war, when my work on the polarization had shown that early-type stars should show polarization at the limb. Since electron scattering must be a dominant part in the continuous absorption of high-temperature stars, it occurred to me that one could detect the polarization in eclipsing binaries, one of which is low temperature, the other of which is high temperature — one should see the polarization. I went around the country, actually, telling the people who were doing photoelectric work to try to detect the predicted effect. In fact I knew Joel Stebbins, who was in Madison, and he was of course the great pioneer in photoelectric photometry. So I asked Stebbins, "Why don't you try to detect this effect." Well, Stebbins was not too interested. Later, I asked Al Hiltner in the department, who was looking out for

some new things at that time — I suggested to him, "Why don't you start photoelectric work and try to find this effect?" He was rather enthusiastic about it, and he joined forces with J.S. Hall, who was also interested in photoelectric things, and they had an observing session in Texas. One of the first evenings, Hiltner called and said they had measured a particular star — I forget, some particular star — the previous night, and had found a polarization. I thought right away that they had found the effect I'd predicted. But then the following day, the polarization was still there, even after the eclipse was over — so it was clear that a new phenomenon had been discovered. That was how the inter- stellar polarization was discovered.* I always say that a theoretician can suggest something to an observational person; in the end, the theoretician gets only the booby prize.

Weart:

The observational person gets the credit.

Chandrasekhar:

Yes.

Weart:

Have you done the same thing with some others of your theoretical predictions, before the hydrodynamics period, in terms of telling people, "Really, you should look for this, you should look for that"?

Chandrasekhar:

The polarization was the one thing I really tried hard to have verified. Let me tell you a little about my relations with Stebbins who was the first person I approached. I had exceptionally good report with Stebbins even though he was much older than me. I have always had a warm feeling towards him. Stebbins was a marvellous person, very modest. I met him for the last time a year or two before he died. On that occasion he told me "Chandra I should have taken you * See W. A. Hiltner, SCIENCE 109 (1949), 165 up and tried to find the polarization you predicted. I should have then discovered interstellar polarization."

Weart:

Well let's see. The RADIATIVE TRANSFER, the book itself, wasn't published until 1950. Is there anything more we should say about it? Was there a distinction between early and late periods of working on the RADIATIVE TRANSFER?

Chandrasekhar:

No. Except, how I discovered Stokes paper, you know.

Weart:

Yes, how did that happen?

Chandrasekhar:

I had done the problem of the plane parallel atmosphere, and there the problem was simple. Because from symmetry, the plane of polarization should be in the meridian plane; I could assume that the plane of polarization is in the meridian plane, and you characterize polarized light by just the two components. Then I wanted to look at the problem of diffused reflection. In that case, the plane of polarization would change all over the place. So the

question is: How do you incorporate the plane of polarization, which is unknown? And all the books on polarization would always tell you that given the two planes of polarization, the intensity varies like a cosine squared plus sine squared, something like that. Now, how can you incorporate in an equation transfer a direction, in addition to two intensities? It seemed hopeless. And I went to many many physicists to ask them how one should characterize polarized light? For example, John Wheeler was here in Chicago. I went and talked to him. No, he couldn't give me any answer. And then I went to Madison, Wisconsin where I knew Gregory Breit. I asked him how one would do that. No, he couldn't tell me. And I talked to George Placzek, whom I used to know from Copenhagen times, a great expert on the scattering of light. And what George Placzek told me was, "Well, Chandra, you have taken one of the most difficult problems. Rayleigh tried to work on the polarization of the sky, to compute the degree of polarization. You have taken a really difficult problem." And Gerhard Herzberg, who was my colleague at Yerkes I used to talk to him. And he couldn't tell me either. Finally one day I really got

upset about this and said, "Let me try the old masters."

Chandrasekhar:

So, having tried to take advice from all the physicists whom I knew, and having failed, I finally decided that perhaps I should look to the old masters, and brought down the collected papers of Rayleigh, Stokes and Kelvin. When I look through Stokes volume, I think it was volume 3, looking through the contents, I saw a title on "The Mixture of Streams of Independently Polarized Light." I turned the page and looked at it. And I said, "That's exactly what I want. I remember calling Gerhard Herzberg and saying, "Here is a paper by Stokes, and the problem is solved. It is absolutely clear." In the paper which I wrote a week or two later opening sections were a description of Stokes work, presenting it in the form in which I needed it. And I called it "The Stokes parameters." At the time I wrote on Stokes parameters, there was not a single book on optics which had an account of Stokes work.

Weart:

It had somehow been lost.

Chandrasekhar:

Just lost. In fact, every one of my papers on the polarization up to that time — I wonder if I said in my book about that? [looks in RADIATIVE TRANSFER] No, I don't say it here, but in my published papers I point out that Stokes paper was in 1852, and just completely lost. You look at Drude's OPTICS, or Born's OPTICS, the earlier edition, and you will not find an account of Stokes) work. But since that time, of course, there have been many accounts.

Weart:

Right.

Chandrasekhar:

A marvelously written article by Stokes.

Weart:

Remarkable. Well, now I wanted to ask about some institutional things. This is about what happened at Yerkes and Chicago, during the same period that you were finishing up your RADIATIVE TRANSFER book. Let me just run down the outline

of the chronology, then I'll ask you what you can fill in on. In 1947, Struve split up his functions. He stayed head of the department, but in July of 1947 Kuiper became the director of the observatories, Morgan became the managing editor of APJ, and you were to conduct a section of theoretical astrophysics — Struve hoped that there would be an institute for theoretical astrophysics — and you would continue to supervise the teaching at Yerkes. This is from reports in the AJ.* The section of the astronomy department on the campus in Chicago would be revived, and Struve would coordinate the activities of all four branches. In 1948, Greenstein and van de Hulst left. In '49, Kuiper asked not to be reappointed as director. And observatory council was formed, for organizational matters, Struve as chairman. Herzberg left. In '51, Struve resigned to go to Berkeley, and you became acting chairman of the department. Phillips left, Page resigned as secretary, and then of course later Strömgren was brought in. * Annual Reports of Yerkes Observatory in ASTRONOMICAL JOURNAL. But particularly, first, up to the period when you became acting chairman, can you fill in on what happened?

Chandrasekhar:

That was an unhappy period. To an extent, I was not deeply committed to any administrative aspects of the department, till this happened. I could perhaps tell it in as objective a way as possible, without involving too many personalities. It's too bad that there is no one now living who could, in my judgement, confirm or support what I say, because the only person who's still living, who was involved in those times, is Morgan. I'm not at all certain Morgan's remembrance of these matters is correct, because there are many things which I know happened, which he either doesn't remember, or he sees differently.

Weart:

This makes it all the more important, then, that we should hear what you recall of it.

Chandrasekhar:

The sequence of events which you said is right, but there is one thing which you did not say, namely, that all these organizations which Struve made — becoming the chairman, appointing Kuiper, and so

forth — happened just a month after I had decided not to go to Princeton. You know, when Russell retired, I was offered the chair at Princeton. And I accepted it.

Weart:

Oh, I didn't know that.

Chandrasekhar:

I accepted it. I was offered the chair in the early summer, maybe June of that year, '47 was that?

Weart:

Yes. This all began in July of '47.'

Chandrasekhar:

I'm sorry — then, I must have been offered the position the year before, '46. I certainly could confirm the date, '46, because that was the year Schwarzschild went to Princeton.* You see, I declined the position, then Spitzer and Schwarzschild were appointed. Anyhow, in the summer of '46 I was offered the job at Princeton, and I had accepted it.

Weart:

I should ask you first why you accepted it?

Chandrasekhar:

I remember that Henry Norris Russell invited me to come to Princeton. I talked to Hugh Taylor, and they all told me that I could do my research there, and they offered me a professor- ship, and — this fact, I think, didn't play a role, but the salary * N.B.

Schwarzschild took up his duties at Princeton _947; the negotiations were in 1946 - SW. they offered me was twice what I was having in Chicago.

Weart:

I wonder whether the difficulty of going back and forth between Yerkes and Chicago may have played a role?

Chandrasekhar:

It wasn't too serious because I'd just started it. That didn't play too much of a role. But then Hutchins was able to persuade me, in September, that I was probably not wise in going to Princeton. He told me the following: "Well, it's an honor to succeed

Russell, but it is more honorable to leave a chair, to which it is an honor to succeed." Then he said, "Of course, we can't provide you with that honor, but on the other hand, you have to ask yourself whether you can really do your work better there." Then he turned around and asked me, "you know, Kelvin was a professor in Glasgow for 50 years. Do you know who succeeded him?" I didn't. At any event, at the same time, he offered me a Distinguished Service Professorship here in Chicago, at the same salary as Princeton was offering. Well, I didn't think the salary was playing a big role, but one couldn't help noticing.

Weart:

There's a good line that one scientist told me once, that in America money has symbolic importance. It shows how you are evaluated by people, and that's what's most important.

Chandrasekhar:

But actually, at that time I was very short of money. For example, my wife very badly wanted to go to India. We couldn't afford the money for her to go. My salary at that time was \$5000. We simply didn't

have enough money, and my wife was deeply unhappy because she couldn't go back to India.

Weart:

For a visit?

Chandrasekhar:

For a visit to see her mother. We simply couldn't afford. the money. So to some extent, this did play a role, I'm sure it did. And so, I declined [the Princeton offer].

Weart:

You mentioned Hutchins, but what about Struve and the physicists and the astronomers?

Chandrasekhar:

They didn't persuade me very strongly.

Weart:

Why did Hutchins step in?

Chandrasekhar:

The only thing I can tell you is that every time I met Hutchins, after he left Chicago — I met him once when he passed through Chicago, there was a large assembly there and I was there. He came over to me and said, "One of the nicest things I've done, which I have done at Chicago which I'll always remember, is that I am responsible for your being here." And last fall, when I went to call on him in Santa Barbara, — he repeated the same remarks. You know Hutchins has since died.

Weart:

yes, I know, just this last weekend.

Chandrasekhar:

I'm very glad I did call on him. I called him on the telephone and said, "I would like to see you and make a call of respect." He was very nice. He said, "Please come and spend some time with me." I went there; I thought I would stay for ten minutes. He talked to me for nearly an hour and a half. Before I left, he again told me what he had told me earlier. Of course, people say these things in politeness, and I

do not know to what extent it was politeness and to what it was not, but he seemed, to the extent I can judge, genuinely pleased that to some extent, he was in fact responsible for my coming to the university. That clipping I showed you, showed that he had over-ruled the dean. Of course, my appointment was recommended by Struve, but nevertheless it was Hutchins who had overruled the dean. And it was use Hutchins who persuaded me to decline the offer from Princeton. It was not the astronomy department which did it.

Weart:

I see.

Chandrasekhar:

It was at Hutchin's persuasion. But I must confess that the fact that he made me a Distinguished Service Professor in '46, when Struve had been made a Distinguished Service Professor only a few months earlier — I don't think Struve liked it. At least, he was so sensitive to honors of this kind that he felt that he must somehow redress the imbalance which had been created. And so Kuiper, who is an admirable astronomer, who certainly deserves

everything — certainly if my getting the Distinguished Service Professor implied anything for Kuiper (I didn't think it did, but if it did), and if Struve sensed it correctly — and Struve might very well have sensed it correctly — he might have wanted some additional recognition. So Kuiper was made the director, and Morgan was made the managing editor of The ASTROPHYSICAL JOURNAL, as you said. But I am afraid that was the wrong thing to have done. was perfectly all right, in my judgement, if Kuiper had been made a Distinguished Service Professor, or his salary increased to the corresponding level. But you see, Struve and Kuiper are different kinds of persons. Struve is a person who likes to have all the strings. He's extremely sensitive to rank and position. I advised Struve as best I could that he should not make Kuiper the director. But it is possible that he misunderstood me as implying that I didn't want Kuiper to be the director.

Weart:

For personal reasons?

Chandrasekhar:

For personal reasons. But anyhow, Struve made it.

Weart:

But there must have been other things reacting. It's a very serious step for someone to relinquish authority. Did he see himself as relinquishing authority?

Chandrasekhar:

He thought that people would constantly consult him, and that his giving up the directorship at Yerkes was rather similar to the way Hale gave up the directorship at Mt. Wilson. Hale gave up the directorship, Adams became the director, but Adams consulted Hale at every point. Well, he thought he was getting a similar situation at Yerkes.

Weart:

And relieving himself of some of the administrative responsibility?

Chandrasekhar:

Yes.

Weart:

And likewise with the APJ?

Chandrasekhar:

Yes. But on the other hand, when you come to the APJ situation, the situation became rather complicated. Let's leave that —

Weart:

We'll leave that aside, yes.

Chandrasekhar:

But anyhow, you see, it is simply a fact that Kuiper and Struve didn't hit it off. And the personal frictions, the annoyances coming between them, was very disruptive, Kuiper was a personal friend of mine; he was deeply unhappy about the situation, and Kuiper and I decided that the best thing was to restore the status quo ante. Therefore, I persuaded Kuiper to resign the directorship, so that Struve could be the chairman, but Struve could not accept it that way.

Weart:

Why not?

Chandrasekhar:

Because he didn't want either in appearance or in fact, that he was anxious to run the whole place. Because he had said originally that he had not wanted to. I am not a psychoanalyst. But it is clear that Struve had for all appearances (thought in fact) relinquished administrative responsibility, and that what was effectively being done was restoring his formal responsibilities is what I wanted to do and that is what Kuiper wanted to do.

Weart:

Why did Kuiper want to do that?

Chandrasekhar:

Because he was not getting any thing out of the directorship He still had to get everything approved by Struve, meant [only] a lot of typing work. To have an administrative job with no responsibilities is a completely futile position for a scientist, particularly for a scientist like Kuiper, who at that

time was in one of his most active periods, doing marvelous work in planetary astronomy. That was one of his best periods. He did many of his major discoveries at that time. He discovered carbon dioxide on Mars. discovered the atmosphere of Titan — the first time a satellite had been found to have an atmosphere; he was doing marvelous things. Why would he want to sit and type letters for Struve? In fact, I tried to persuade Kuiper at an earlier time that he shouldn't become the director. But he did not take my advice. It wasn't working, and I knew it wouldn't work.

Weart:

What did Kuiper think of Struve?

Chandrasekhar:

I always felt that Kuiper and I and everybody else in the department had enormous respect and admiration for Struve. But so long as Struve had the complete administrative control, and we did the science, and he encouraged us to do science and never interfered with our scientific work, it was an ideal setup for us. So why would we complain? We were not competing for any administrative distinction. None

of us were. Kuiper was doing his best work, and I was working on transfer theory, which was my happiest period, and Morgan was doing his work, certainly the best of his life, at least the most recognized of his work. And we were all perfectly happy.

Weart:

It raises an interesting question. Why should you all be doing good work at exactly that period?

Chandrasekhar:

We were all in our primes.

Weart:

A coincidence?

Chandrasekhar:

No, I would say that unless one happens to be exceptional like Fermi or Heisenberg or people like that, I would say the best scientific work a person does is between 30 and 40. Because he has passed through the stage of apprenticeship, he's on his own, he's full of strength, full of ideas, full of optimism,

feels his whole career is ahead of him, feels all the strength he has.

Weart:

Just that you were all at that age.

Chandrasekhar:

Yes, we were all in our middle thirties. And we were all entirely satisfied. I think it was a terrible mistake to have changed administrative direction. But on the other hand, you see, Struve was the one person who, retrospectively it is certainly right to say, had reached his top in science. From that time on it was a decline for him. He must have sensed it, bitterly. But I think at this time it's quite clear that Struve's best work was behind (in the late forties). We restored the status quo ante. But Struve was not very happy. He left for Berkeley.

Weart:

Why was he not happy?

Chandrasekhar:

To quote a remark which Kuiper said in one of his moments of anguish (which is probably unfair, both

to him and Struve, to say), Struve was all the bad qualities of the Russian, and the bad qualities of the German, with the good qualities of neither. But the fact is that as you know, Struve must have been a very unhappy person. You know, his wife died after having melted all his gold medals.

Weart:

No, I didn't know that.

Chandrasekhar:

His wife died after him; no one knew when she died, but she had melted all his gold medals. All his books were sold. All his papers were destroyed.

Weart:

His personal life was unhappy.

Chandrasekhar:

I'm sure it was.

Weart:

And then he felt his colleagues did not appreciate him.

Chandrasekhar:

He thought that they didn't appreciate him. You know, if you have a colleague whom you admire, you can't go and tell him all the time that you admire him. I have a young colleague in relativity, Bob Geroch, who I think is absolutely first rate, but that doesn't mean I tell him all the time he is wonderful. That's just not possible. But Struve was very sensitive. And somehow or other, he felt that his position in the university was not as strong as it was before that. He was a protégé of Hutchins. But after the war, there was Fermi, there was [Carl-Gustaf] Rossby, there was Urey, there was Libby, there was Maria [Goeppert] Mayer, there was Edward Teller — and Struve was only one of the group.

Weart:

I see.

Chandrasekhar:

And he did not feel that he was very happy. For example, Walter Bartky was made the dean after Compton. Well, Struve was the one person who objected strongly to Bartky being made the dean. I

think, to be entirely fair to Struve, one must say that he had scientifically passed his prime. He was no longer the great man at the University of Chicago (which he was). Up to 1946, [Arthur Holly] Compton was the only other person whom one could have put superior to Struve. Well, the climate had changed. He no longer had the same access to Hutchins which he had before. And he no longer was the undoubted intellectual leader of the astronomy department. The faculty which he had brought had grown and matured and were making reputations of their own, so that no longer was the astronomy department Struve and the rest, but Struve plus X, Y and Z. He was probably not comfortable with that. And he certainly went to a department, in Berkeley, in which during his lifetime there was Struve, and no one else. These are not qualities which are uncommon among scientists.

Weart:

No, it's not.

Chandrasekhar:

And I personally had then, and have now, the highest regard for Struve. My own feeling is, if

Struve had not given up his chairmanship at that time, certainly there would have been no change in the attitude of his colleagues towards him, and he could have been happy the rest of his life. Indeed, he might have been more happy than in fact he was in Berkeley, and certainly, than he was at the National Radio Observatory.

Weart:

One thing we didn't mention in passing was the business about reviving the astronomy department here on the campus.

Chandrasekhar:

It essentially meant only one thing — that Struve started to give a course of lectures on the campus. But very few people came, and Struve always gave his observing periods the highest priority. He used to go to McDonald every few weeks, and lecturing was not possible. Consequently, I took the teaching on the campus over, and that is how I started giving regular courses on the campus; after 1946, regularly every year, I was on the campus every Thursday and every Friday, from 1946 to 1964.

Weart:

This was the time Wilson speaks of,* when you had a class of only two: Lee and Yang?

Chandrasekhar:

That's right.

Weart:

What class was that, what were you teaching Lee and Yang?

Chandrasekhar:

That was in the period 1947 to '49.

Weart:

What sorts of courses did you teach?

Chandrasekhar:

In fact, that particular course was on the theory of stellar structure. And T.D. Lee did a thesis on white dwarfs, under my supervision.

Weart:

I see, I hadn't realized.

Chandrasekhar:

When people ask me about my former students, till recently when the story with Lee has been widely publicized, I've never included Lee among my students. Like an old story of Maupassant, where there was a woman whose son became a Pope, and this woman was sent to a lunatic asylum because she claimed that the Pope was her son. Seriously, I mean, T.D. Lee is a marvelous physicist, and I don't claim any credit for him.

Weart:

It was mentioned here that you became the director of a * "Introductory Remarks" by John T. Wilson to S.C. "Shakespeare, Newton, and Beethoven, or, Patterns of Creativity," Ryerson Lecture, Univ. of Chicago Center for Policy Study, 1975. theoretical astrophysics section?

Chandrasekhar:

It never materialized. I never took it seriously, because I didn't see that it made any difference; students doing theory were doing work with me, and

what's the particular point in calling it by some name?

Weart:

Then you became acting chairman for a year, and you also chaired the council of astronomers that picked Strömngren to come as director. One of the most interesting things is how it was that Strömngren was picked, and also the history of your relationship with him.

Chandrasekhar:

Actually, the council hardly did very much on it, because Struve had recommended Strömngren to Hutchins; I'm afraid the appointment, the decision to make Strömngren the director, was made by Hutchins and the dean. The astronomy department simply approved what in fact was an administrative fait accompli.

Weart:

I see. I wonder, by the way, when you first met Strömngren and what the history of your relations with him was. Did you see him at Cambridge or Copenhagen, whatever?

Chandrasekhar:

My attitude in the thirties and forties was always one of conceding priorities to all the others. In 1932, when I met Strömberg, I thought Strömberg was a great astronomer. In 1946 when he visited Yerkes, one of his famous papers he left half unfinished. I wrote it all up for him and published it, you see. There's no doubt that he's absolutely an excellent astronomer, perhaps of a rather conventional kind, but still. And when he was made the director by Hutchins, I thought it was a very good thing. I admired Strömberg sufficiently to think it would be a very good appointment. But on the other hand, thinking back, there are instances in which retrospectively I can see that if I had the normal attitude to my colleagues and contemporaries that anyone would have, I might have had a different view.

Weart:

To finish up the story of Yerkes, you did mention, and I think it's quite clear, that the period you spoke of when you, Morgan, Kuiper and all were all doing excellent work was one of the high points for

Yerkes, and then it went down into quite a long trough. I wonder if you could comment on why you think that happened? What were the factors in it?

Chandrasekhar:

My own judgement is that the choice of Strömberg as director at that period was a mistake. He was not the right person, and administratively he was extremely bad. That is not only my comment. For example, when he was the president of the IAU the general secretary told me that Strömberg never replied to any letter which was written to him. And during the time he was director, the first and second year, he used to go away for three months to Copenhagen. When he accepted the job at Yerkes, he retained the directorship at Copenhagen, so every three months he was to go back to Copenhagen — during the first few years anyway. Which was not known to me; I was completely and totally astonished that he had made these arrangements with the administration without any information, to me or to others. For example, during the summer he was gone I was the acting chairman again, and there were letters in his files six months of letters, completely unanswered. Moreover, some of the younger people

who came to Chicago at that time, whom I had appointed during the one year in which I was acting chairman — Aden Meinel and Harold Johnson are the two men I had appointed as assistant professor, during that one year I was the chairman — and people whom Strömgren brought later, like Dan Harris, and Adrian Blaauw — all these people, I mean, decided that the nature of graduate instruction had to be different.

Weart:

In terms of the scientific program?

Chandrasekhar:

Everything, yes. For example, they revitalized, as they said, the graduate program, and it was all done without my knowledge, even though I had been responsible for it up to that time. There was a departmental meeting in which the whole new program was brought up, and I wasn't aware that it was going to be brought up. It was brought up at the last minute, the meeting was going on and on and they weren't coming to this point. I had to leave and Strömgren asked me for my comments. I made the remark that if they changed the program as they want

to that was all right with me, if that was what the department wanted, but it was clear to me that the program had been so arranged that it would not be possible for me to have any more students in the astronomy department, because they would not be prepared to work with me. And that to some extent, they would have to carry on the program themselves, and I shall find my avenue of teaching in other sections of the university. Then I became a member of the physics department, and started teaching in physics in the physics department.

Weart:

That was in?

Chandrasekhar:

'54 or '55. After '54-'55, I have not taught in the astronomy department.

Weart:

Is this because they were moving still further away from theoretical?

Chandrasekhar:

That is what they said. Of course, you know, the department is totally changed now. But I think up to a point, Strömngren probably felt, probably with justice, that I was not in sympathy with the way in which he was conducting the affairs of the observatory, and he wanted to have a free hand. After all, if a man has a responsibility, and he feels that someone within the department is not in sympathy with him, I think it is right on both sides that they do not interfere with each other's responsibilities. I think he was probably right, from his point of view, to see that I had no influence in the department. And I was, I think, right from my point of view, to give up my active relations with the astronomy department at that time. And moreover, my own interest was not in astronomy at that time. It was shifting to the campus.

Weart:

Was this happening before these events?

Chandrasekhar:

It was all simultaneous. I realized what was happening in the department, up to a point, was in my interest. I probably would have continued my teaching in the astronomy department had this not happened. But on the other hand, it gave me a very good reason to get myself relieved of conventional shakes. And the department wanted a change in direction. On the other hand, I'm sure that Strömberg was doing what in his judgement was right and what in the judgement of the rest of the department was right. Nevertheless, he was not committed to the observatory, and the department, because he left the university in '57 or '58. In any event, no department can stay at the peak for ever. For example, take the physics department. There was Fermi; after Fermi died, and Maria Mayer left and Urey left and Libby left, well, the department declined. Struve was the person who held the whole department together. He had created the department, he had brought the faculty which had made the department during the thirties and forties. When he left the department it lost cohesion. Perhaps it was the natural course of events that it went down. I certainly don't want to give the impression that somehow or other people

willfully tried to undermine each other, which was not the case.

Weart:

No. I think it's important to know why it is that an institution goes up, and why it should go down. I've often wondered whether, in the case of Yerkes, the pull of the 200-inch, and the beginning of various other observatories, had something to do with it.

Chandrasekhar:

That is part of it, certainly. Certainly in the late forties, the only place where a graduate program in astronomy was carried out, in the way in which it is carried out since in other places, was Yerkes. That was the only place which did it. And then when Struve went to Berkeley, he had Henyey there, and they developed a graduate program rather similar to that Yerkes? Greenstein and Guido Münch went to Cal Tech, they developed a program rather similar to what we had in Chicago. Lawrence Aller at Michigan developed a program rather similar to Chicago, because he came to Chicago all the time. And Schwarzschild and Spitzer [at Princeton] of

course didn't follow the Chicago pattern, but by necessity it was similar to it.

Weart:

I wonder if you can tell me, since you had such a part to play in establishing this, what were the major elements that you saw in this pattern, that only Yerkes had originally?

Chandrasekhar:

Chicago was the first to realize that a graduate program in astronomy must include a course of instruction in the major fields of astronomy. And the major fields of astronomy, as we defined them in the late thirties and the early forties and the middle forties, were: stellar atmospheres, two or three courses a year; two or three courses in interstellar matter; a course in atomic physics; a course in molecular spectra; a course on galactic structure; a course on stellar dynamics; a course on the solar system; a course on stellar spectroscopy — these were the staples that we provided. We had a system of courses, in which all these were represented.

Weart:

Why was it that that was done at Yerkes, why there, why at that time?

Chandrasekhar:

I think primarily the credit must go to Struve. Struve realized that it had to be done. He himself couldn't do it, because he was too occupied with administrative and other matters, and he asked me to do it.

Weart:

Had he sketched out the plan already, in this form?

Chandrasekhar:

I don't think he sketched out the plan. The plan was largely set up by me, in collaboration with Kuiper and Struve, but Struve gave complete freedom to develop a program as I wanted it. The program necessarily reflected my interests — namely a very large part of it was theoretical. That had to be changed, and Strömgren and others changed it, and restored a balance to it, which is what we have now.

Weart:

I see.

Chandrasekhar:

I think the events that took place were natural, with my background. I was given charge, Struve had an interest to develop a program, and I being in charge, developed a program which was largely theoretical — which obviously wasn't right.

Weart:

But Struve had simply seen that the students were not getting proper preparation?

Chandrasekhar:

That's right.

Weart:

Now, perhaps, because we don't have a lot of time, maybe it's time to turn to the APJ. You joined, was it in 1944, as one of the editors?

Chandrasekhar:

Yes! As Associate editor.

Weart:

Associate editor. And perhaps, even before we get to the postwar period, I'd like to ask you a little bit about being associate editor then, how was it different from what it is today? What was it like then, what was Struve's role?

Chandrasekhar:

ASTROPHYSICAL JOURNAL, after 1953 when I took it over, was totally different from the JOURNAL before, because in 1953, the University of Chicago signed an agreement with the American Astronomical Society, so that the ASTROPHYSICAL JOURNAL was sponsored by the Astronomical Society, and there was compulsory subscription.

Weart:

I was just wondering what an associate editor did.

Chandrasekhar:

So, let us carefully distinguish the ASTROPHYSICAL JOURNAL after 1953, and before 1953. When Struve was the editor of the

ASTROPHYSICAL JOURNAL, it was primarily a journal in which two observatories participated for their publications, the University of Chicago and Mt. Wilson. Both of them could publish in the ASTROPHYSICAL JOURNAL to any extent they wanted — no referee, nothing.

Weart:

So what did an associate editor do?

Chandrasekhar:

Let me come to my part in that.

Weart:

Oh, OK.

Chandrasekhar:

That is how it was in Struve's time. Struve became the editor in '35, I believe — maybe in '32.* I will come back to that in a moment. During Struve's period of editorship, up to about 1944 when I became associate editor, it was a private journal, in place of the observatory publications. That was that it was. But in 1944, Struve wanted to widen the base of it and increase the participating observatories

from two — (it was in fact three at that time, Yerkes, McDonald and Mt. Wilson, but really in effect two). He widened the base by including Harvard and Lick. And the same thing was there. These four observatories had a right to publish anything they wanted, up to a certain maximum number of pages.

Weart:

How did he get Harvard and Lick to agree to this arrangement? Harvard had its own Bulletins and so forth.

Chandrasekhar:

Well, Harvard came in. It's a very delicate question to ask. There were obvious scientific advantages to publishing in the *ASTROPHYSICAL JOURNAL*, because Donald Menzel was writing papers in astrophysics. Menzel started publishing his papers, for example, in the *ASTROPHYSICAL JOURNAL* prior to this arrangement. And obviously, it was in Menzel's interest to go along. Let me just put it that way. Let me not go into psychological problems. Similarly, C.D. Shane became the director of Lick very soon after that, or just about that time. And they felt that the observatories were now doing things

which go better in a JOURNAL than in observatory publications. * In 1932 - SW.

Weart:

I wonder whether the fact that the war was on, that there were not so many publications, may have played a role?

Chandrasekhar:

To some extent. But actually, Struve was a very far-seeing man. He felt that sooner or later the ASTROPHYSICAL JOURNAL, or something equivalent to it, had to play a role for American astronomy which the PHYSICAL REVIEW was playing [for physics]. That was Struve's idea. He knew that he couldn't do very much along that direction before the war ended. But Struve talked to me about it. That was the first part. And the second part was, an increasing number of theoretical papers were coming into the JOURNAL, submitted by various people from the participating observatories; and, for example, H.R. Robertson was writing papers on cosmology from Cal Tech, which came in as Mt. Wilson publications. And there were papers by Ira Bowen on spectroscopy. So Struve was

anxious that he had some one who could read the theoretical papers, and help him. So when I became an associate editor with Struve, I read all the theoretical papers which came in.

Weart:

What was involved in editing, if you had to publish them anyway?

Chandrasekhar:

Well, but I read them, I had the chance to eliminate some obvious errors, and things of that sort.

Weart:

If they were wrong, you would say, "Listen, you don't want to publish this error"?

Chandrasekhar:

Well, I'd write them a personal letter. There was no referee. If the author said, "I won't change that was that. I will come back to some of these remarks later. So that was Struve's idea. But then, he was the president of the Astronomical Society after the war, and when he gave up the editorship of the JOURNAL and Morgan became the managing

editor, Struve was extremely anxious that the *ASTROPHYSICAL JOURNAL* should get a national base. And since he wanted to look into that matter, he wanted to give the day-to-day running of the *JOURNAL* to Morgan. Because at that time, the *ASTRO- PHYSICAL JOURNAL* was a press journal, which means it was a private journal published by the University of Chicago, paying no overhead. Then Struve formed a committee in the Astronomical Society to look at the problem of publications; Lyman Spitzer was the chairman of the committee. Lyman Spitzer, [Dirk] Brouwer, Paul Merrill, and Nick Mayall were the people who were the members of this committee to look into this problem. This committee met, and it produced its final report the year in which I was a acting chairman.

Weart:

1950.

Chandrasekhar:

Yes. The key point here is, Struve felt very strongly that there was no national medium for publishing astronomy, and that a private journal like the

ASTROPHYSICAL JOURNAL simply would not do. And since he was the president of the Astronomical Society at that time, he formed a publications committee, consisting, as I said, of Lyman Spitzer from the East, director at Princeton at that time, Paul Merrill, who had been on the [APJ] editorial board all these years and was editor of the MT. WILSON PAPERS, and Nick Mayall from Lick, which was a participating observatory, and Brower from Yale. These were the members of the committee. Now, during 1950 I was acting chairman, and Morgan said that the carrying through of the program of the Astronomical Society, and its negotiations with the university must be carried on by me, because I was acting chairman. And I did. That was a year of great changes, because Hutchins left, end of '51. He left the year that Strömgren came. But Hutchins was still the chancellor in 1950. I know that because the appointments of Meinel and Johnson, I negotiated through Hutchins at that time. But Caldwell was the president, and he was in charge of these other matters. So an agreement was to be made with the Astronomical Society.

Weart:

Well, assuming that the Astronomical Society was in fact in favor of this.

Chandrasekhar:

In fact, Lyman Spitzer's committee had come out by saying that the Astronomical Society would sponsor the ASTROPHYSICAL JOURNAL and the ASTRONOMICAL JOURNAL, provided an agreement could be signed with the University of Chicago Press.

Weart:

And this had to be approved by the AAS Council, I suppose.

Chandrasekhar:

Yes, by the Council. And it had to be approved also by the university. Now, there were a lot of cross currents at that time, because many of the stalwarts were rather against the ASTRO- PHYSICAL JOURNAL becoming the national journal, because it would mean giving a private journal supervision and authority over the astronomy of this country, and

why should a private journal have that authority? In particular, why should Chicago have it? They might have been willing to accept Struve, but Struve was not in the picture any more. So there was a natural feeling against that. But anyhow, Spitzer was young and he was able to persuade the committee to go along with writing a contract with the University of Chicago.

Weart:

On speculation, so to speak.

Chandrasekhar:

Yes. And I was in charge of making the contract. These letters, incidentally, are in my files. I won't go too long into this matter, but the main point of the agreement was, the managing editor and the associate editor will have to be members of the University of Chicago faculty. That was one of the stipulations in the contract. There were others, you see. Anyhow, I wrote up this memorandum of agreement, which I had talked about, not very much, but a little, to Lyman Spitzer.

Weart:

Had you worked it out with Hutchins, on this end?

Chandrasekhar:

No, I just tried to make it out on my own, as what was in the best interest all concerned. I made it out, and of course I showed it to Morgan. Morgan during that year gave me complete approval to do anything I wanted. So pretty nearly, I carried the ball. I wrote the memorandum, and Caldwell was to forward this memorandum to Alfred Joy, who was then the president of the Astronomical Society at that time. And Caldwell had a covering letter which was manifestly rude. He said the University of Chicago was willing to go along, and so on and so on in a very luke warm style. But the fact of the matter was, it was in the interest of the **ASTROPHYSICAL JOURNAL** to be sponsored, because Americans will not accept a private organization. They will be willing to accept a national organization. (Caldwell never recognized it)

Weart:

Of course. Was it purposely rude?

Chandrasekhar:

Well, I don't know whether it was purposely or not, but the letter was in fact rude. In fact, I did not see the letter. He just forwarded it on his own. And at the December, 1950 meeting of the American Astronomical Society, this letter was considered by the Council. The Council rejected it flat. They would have nothing to do with the ASTROPHYSICAL JOURNAL.

Weart:

And you think it was because of this letter?

Chandrasekhar:

That's right. Joy wrote a letter to Caldwell saying that the letter which Caldwell had written was unacceptable to the Council, and the Council had dissolved the publications committee, and they would not sponsor the ASTROPHYSICAL JOURNAL. They did not want to have anything to

do with it. This letter came to me in January, 1951, when I was no longer the acting chairman.

Weart:

Had you known this, or did this letter just come?

Chandrasekhar:

Just came. From the blue.

Weart:

You hadn't been on the telephone to people to talk about these things?

Chandrasekhar:

No. I was completely horrified.

Weart:

You had expected, in fact, the AAS to accept it?

Chandrasekhar:

Of course, when I saw Caldwell's letter, I couldn't see how they could accept it. I was horrified. Then, you see, in February 1951, we were going to India.

We had been in the U.S. 14 years, and I told you earlier that my wife was desperately anxious to go.

Weart:

And you hadn't been able to go back all that period.

Chandrasekhar:

1951 was the first time. I could pay \$2400 to get the ticket, because round-trip tickets for both of us were \$2400, and in those days \$2400 was not small, you know.

Weart:

No, it was a substantial part of your income.

Chandrasekhar:

For the first time, I could afford it.

Weart:

Both of your families were in Madras?

Chandrasekhar:

That's right.

Weart:

Had your families been affected by the troubles of that period? It was a very difficult period for India, after all.

Chandrasekhar:

Oh yes.

Weart:

So that was part of the anxiety?

Chandrasekhar:

Well, in the case of my wife, her mother was not well, and she badly wanted to go, and she was rather lonely in this country.

Weart:

And you had not seen your parents, your father —

Chandrasekhar:

No. I hadn't seen my father, you see. But anyhow, this was the first time we could afford to go. And so we had planned to go in February. This letter from the Society came about two or three weeks

before I was to leave from India. Morgan said, "It's your baby, you take care of it." Martin Schwarzschild is a very good friend of mine. He has always been a marvelous friend. I talked to him just last ight a long time over the telephone. And I wrote to Martin, "We are going to India, but we would like to stop in Princeton the day before, and I should like to discuss the *ASTROPHYSICAL JOURNAL*, with you and Lyman. So on Sunday, the first Sunday in February or the last Sunday in January, 1951, Martin and Lyman Spitzer sat on one side of the table, I sat on the other side and said, "Lyman, you were the chairman of the committee. You recommended the rejection of the proposal on the basis of Caldwell's letter. Now, I did not write Caldwell's covering letter, but I wrote the memorandum which follows it. Now, let us forget about Caldwell's letter for the moment and ask for the substance and weight the memorandum the way it is. Let us forget anonymity and so on: I wrote that. Now, what are the reasons?" So I placed on the table all the reasons why the University of Chicago had to keep the editorial control.

Weart:

And what are those reasons?

Chandrasekhar:

By and large the reasons were, the JOURNAL was financially at that time supported partially by the university. That support would have to be forfeited. Also it had to have an organization behind it. And to the extent that the JOURNAL was printed in Chicago, and to the extent that the responsibility lay here, we ought to have free control. The background points were that the astronomical community outside had no confidence in Chicago. Or at least, pretended it didn't have any. I'll give you one example of how this came out in the very end. For example, it is well known that Harvard was completely and totally against the Chicago group, scientifically. For example, every year Shapley used to publish a "HIGHLIGHTS OF ASTRONOMY." Try to find one Chicago discovery in that, during this period.

Weart:

Ah. I never noticed that.

Chandrasekhar:

I think it is important, looking at it retrospectively, not to confuse main issues and personal issues. I think the principal basic fact is that at Chicago, they were all young astronomers in their late thirties and early forties, and a national journal controlled by them is something which other institutions can reasonably resent. Why is that argument not sufficient? That is a basic argument, and I understand that and appreciate it. But on the other hand, from the point of view of the astronomers at Chicago, and me in particular who was conducting the negotiations, I had to see the facts. I knew that we had to be responsible. And if you are going to be interrupted by personal differences or personal irritations, it's not going to work. Lyman is a sufficiently worldly man to understand these things. So I explained these problems to him. And then Lyman, at the end, said, "What do you want to do?" I said, "I have explained the whole problem. You write out the conditions under which Chicago should sign a contract with the university." He said, "All right, I'll write one." That evening there was a party at the Lyman Spitzers. He gave me a longhand memorandum; and that memorandum is exactly

what was in the statutes, the standing orders of the Council, a year later. An incredible fact — a chairman of a committee which had been abolished and an acting chairman who was no longer a chairman, agreeing on a certain contract. And next day I had to leave from La Guardia [airport]. I sent a copy of this memorandum to Morgan, telling him that these were the facts, and if he disapproved of it, he should write to me. And that I expected a letter from him in England, where we were staying for a few weeks, before we were to go to India. Morgan sent a cable to me, saying that he approved it.

Weart:

What were

Chandrasekhar:

The statutes were essentially that the managing editor and associate editor will be members of the University, but Lyman added that there should be an editorial board of five, nominated by the managing editor and approved by the Council. In other words, the managing editor MI! nominate a slate, the whole editorial board, but he will nominate two for each place instead of one as I had originally written. In

other words, I still keep the editorial control, even with regard to the associate editors. That was how it was arranged. That was Lyman's idea.

Weart:

But the Council has a veto over it.

Chandrasekhar:

The Council has a veto.

Weart:

And this he simply thought up that night?

Chandrasekhar:

That night. Lyman is a very wordly person, very able, can see points through.

Weart:

Did you discuss this with him? Did you negotiate, or did he simply say —

Chandrasekhar:

No, after I talked to him, he heard my whole story and said, "What do you want me to do?" I said, "What kind of a contract would you sign?"

Weart:

Then he went off and —

Chandrasekhar:

Wrote it up. And he gave it to me, and I read it and said, "OK".

Weart:

What about the University of Chicago when they saw it? Was it only Morgan, or was it — ?

Chandrasekhar:

Let me go on with the story.

Weart:

OK.

Chandrasekhar:

Now there were two things to be done. I had the responsibility of steering this memorandum through the University. And Lyman Spitzer had the responsibility to reconstitute the committee which had been abolished. Well, Lyman did a magnificent job. He reconstituted the committee, showed this memorandum to Joy and others, and essentially they agreed that they would approve this thing. And when I came back from India in April, I had the task of seeing that it went through the university. Wendel Harrison was the vice-president at that time, (because Caldwell had left), and Kimpton was the chancellor, Hutchins having left, and so I had to negotiate with Harrison. Morgan gave me the complete go-ahead. "You just do what you want." But I gave all the information to him as it went along. By mid-July Harrison approved of it, and the memorandum was approved. I wrote to Joy saying that the University would sign it, and that if he was in agreement, the University (would send a formal letter to them.) Joy said, "Fine." The Strömberg went to Europe, and in October I get a letter from Joy saying, "Why hasn't the University done anything about the letter you said will be forth coming?" I

went to Strömngren and said, "What happened?" "Oh, Harrison wasn't sure that he should sign the memorandum." I said, "Why haven't you told me?" Well, that's just the way Strömngren operated.

Weart:

I see. I've noticed that you've been mentioning you sent the things to Morgan rather than to Strömngren.

Chandrasekhar:

It was because Morgan was the managing editor.

Weart:

But Strömngren was director already by that time?

Chandrasekhar:

Oh yes, but it wasn't deliberate that I didn't tell Strömngren. I just assumed that if Morgan was to do the thing — I mean, he was the managing editor, I tell him, and that was that. So I was astonished. I came to the university here, I was just absolutely white hot. I went to Harrison and said, "Why haven't you sent the memorandum?" He said, "Well, the press has some reservations. The press feels that it

can't support the journal with its editorial authority divided."

Weart:

Can't support it financially?

Chandrasekhar:

No, you see, if I was the managing editor and the complete control formerly was with me, then they could have me removed, or chastised, because both the press and I are under the control of the chancellor. But if the associate editors are voted in by the AAS Council, and the University has no control on them, the press doesn't have the complete control. Because the associate editors cannot be ruled by the University. So they didn't want it. Or: the statement was made that the annual cost will be approved by the Council, and the page charges which I made will have to be approved by the Council.

Weart:

Which Spitzer had also added on?

Chandrasekhar:

Yes. And I approved that, because I think it is fair that if the JOURNAL is supposed to become a national journal, its policies in principle must be approved by the Council. I thought that was right. But the press was against it, and Harrison was against it. As an afterthought. I told Harrison, "I'm sorry, Mr. Harrison. You told me in June that you will approve this memorandum. I have written to Joy, the president, to say that the university would approve it. And that committee which had been abolished by the Society was reconstituted and has approved this, on my word. And now you tell me that you won't approve it! "There are only two courses open: either this memorandum, as we approved, goes to Joy today or, I resign my position from the University. There are no alternatives." He said, "Well, you know, the press has to think about it." I told him, "The director of the press is under your orders. Call him to come here." I said, "This is final — either I leave this office with your approval and your signature on this memorandum, or I call your secretary and dictate my resignation from this university. I am not bluffing. That's that." Harrison said, "You're a difficult man." I said, "You have put

me in a difficult position." Well, the director of the press came, and they raised all these points. I said, "Why don't you leave it to me? You want the ASTROPHYSICAL JOURNAL to become a national journal. We are going to referee papers. We are going to levy page charges. Why would the astronomers of this country give page charges to the University of Chicago? It's absurd. Why would the American astronomers accept my personal responsibility to referee, if I am not in some sense an officer of the Council? In your long-range interest, you will formally lose some priorities of a legal basis, but you have to go with it." Well, Harrison signed the memorandum and it was sent to Joy the same day. I'll tell you an incredible thing which happened. By great good fortune, Schwarzschild and I were both members of the Council at that time. So I could play the strings from both sides. I could play the spokesman for the university in the Council, and I could play the spokesman for the Society with the Administration. Joy was absolutely magnificent. I mean, he was wholeheartedly behind the ASTROPHYSICAL JOURNAL, and at the general body meeting it had to be approved. The Council had approved it, and it had to be passed by the whole

general body, because compulsive subscription was one of the items. And people are not going to pay compulsive subscription We had planned it all, and Joy told me, "Chandra, you must be completely in the background." I said, "OK, I will go and sit in the last row, and I won't say a word." We had all arranged who would speak and who would not speak and everything. And I noticed that Joy looked terribly terribly nervous. I couldn't understand why. After the meeting was over, and all the statutes had been approved by the general body, with half a dozen dissenting votes or something. Joy showed me a telegram, five pages long, from Harlow Shapley. Harlow Shapley wanted Joy to read this letter to assembled members in which. Shapley recommended that the Society does not go along with sponsoring the ASTROPHYSICAL JOURNAL.

Weart:

You don't happen to have a copy of that telegram?

Chandrasekhar:

No. You know what Joy told me? "The telegram came too late."

Weart:

Do you think the telegram would have made a difference?

Chandrasekhar:

Certainly! There would have been so much wrangle and the propositions would have failed. You know Shapley had strong support.

Weart:

Things went through the Council smoothly and at the meeting it just went through on the tracks.

Chandrasekhar:

On the tracks.

Weart:

And that would have thrown it off.

Chandrasekhar:

Yes. That was in December, 1951. I came back to Chicago. There was one other thing which we had to agree, namely, Paul Merrill had insisted that the ASTROPHYSICAL JOURNAL should publish

supplements. For publishing tables and things of that sort.

Weart:

This was because it was taking over some of the functions of the observatory bulletins?

Chandrasekhar:

That's right. So I had agreed to that. And when finally the memorandum was signed, Morgan was furious.

Weart:

About that aspect of it?

Chandrasekhar:

Yes. He essentially told me that I had sold the JOURNAL down the river, by giving so much authority to the Council.

Weart:

I don't understand. He had seen this memorandum. But afterwards he had second thoughts?

Chandrasekhar:

Right. I was rather upset. I told him, "Well, Bill, you have seen everything; you never objected to anything. You told me I had the ball. I've done what I thought was best. It's your job now." I was still the associate editor. Of course, Morgan was not happy with that. But then it happened that that in spring I was for six weeks on the West Coast. I had been awarded the Bruce Gold Medal. I went to Berkeley, and then I was in Pasadena where I saw Paul Merrill. Soon after I returned to Chicago in April of '52, there was to be a first meeting of the editorial board. The first meeting of the editorial board consisted of Lyman Spitzer, chairman, Paul Merrill, Gerhard Herzberg, Nick Mayall, and Fred Whip+. Anyhow, it was to meet. When I came back from Pasadena in April, I found that Morgan was completely uncooperative. He wouldn't talk to me, and he said that his scientific work was being deranged enormously with the editorial responsibilities, and now I had added the SUPPLEMENT which he didn't want to publish. He wouldn't go along with it. And so on. The first meeting of the editorial board was to consider, among other things, how to divide the material between the ASTROPHYSICAL

JOURNAL and the ASTRONOMICAL JOURNAL. And the editorial board met in Chicago. Morgan did not attend the meeting. He asked me to attend, and he had a letter for the chairman of the editorial board, which he want me to give him. He was doing all this by correspondence, even though we were in the same building. He was not well, in many ways; I ought to say that. In fact, he was ill for quite a period after that, probably in consequence, kind of reaction. At the editorial board meeting the editor had to write, which papers in the ASTROPHYSICAL JOURNAL during the past five years did not belong in it. In Morgan's list, of the 30 items he had listed, 20 were my papers.

Weart:

This was in the letter which he gave you to —

Chandrasekhar:

— to give to Lyman Spitzer. And Lyman looked at it; everybody was embarrassed they essentially put it aside, didn't do anything about it. And so when I came back after the meeting, I went to Morgan's office and told him that it was quite clear that he was dissatisfied with the way I had written the contract,

but on the other hand, I had done it fairly, in the sense that he had copies of every correspondence. Apparently the end product is not to his taste. There's nothing I can do about it. On the other hand, I could see very well that being the managing editor, with me as an associate editor, was not satisfactory. Since he had the responsibility to carry on, I would resign my associate editorship. He said, "Will you put that in writing?" "Oh, certainly." So I wrote a letter to Strömngren saying that I was resigning the associate editorship. I told Strömngren, when I gave him my resignation, that Morgan should be absolutely reassured that I will not interfere with him in any way, what I had done I had done in good faith, and if I'd done something wrong, something which could have been avoided, I knew I had not been warned at any stage. That's all there is to be said about it; I resign. And my resignation was accepted on Monday.

Weart:

By Strömngren?

Chandrasekhar:

By Strömgren. Because the Journal was still a part of the astronomy department at that time.

Weart:

This was before that transition had been made.

Chandrasekhar:

That's right. So, Strömgren accepted it; that was on a Monday. On Wednesday, Morgan resigned his editorship. And I found myself the managing editor on Friday.

Weart:

Appointed by Strömgren?

Chandrasekhar:

No. It was quite clear that I had to take it. Dean Bartky asked me if I would take it. And I took it. There was no choice for me, because I had carried the ball — with the Society, with Lyman, with Joy. And I had negotiated on behalf of the university. And if at the end of it, the university says, "The managing editor has quit." Well, how does it look?

Weart:

I see.

Chandrasekhar:

And so I found myself the editor. I immediately instituted all the changes which I wanted to do, refereeing, page charges. I had initially terrible difficulties. One of the first papers that came from Lick — unbelievable as it may seem — it was on binary stars, I had Russell referee, and Russell found a mistake. And Russell rewrote the entire paper. I returned the paper to Lick, saying the paper should be revised. I got a letter from Shane saying, "We at the Lick Observatory know what papers to publish. I suggest that you publish it as we sent it." I wrote back saying, "I won't." I said the paper was rejected. Lick never sent a paper to the JOURNAL for the next two years. And the first time the page charges went, Ira Bowen wrote a letter saying, "We are paying so much pay charges." And I wrote to Bowen, saying, "But look at the money you have spent on your 200-inch. Look at that money you're paying your staff. Look at the amount of time you've spent. Last year you paid only \$500 page charges —

is that that much?" I got a terribly nice letter from Bowen saying, "Please destroy my earlier letter." Bowen was marvelous. The first time a paper from Mt. Wilson was rejected, that member of the faculty sent a petition around, saying that this was the first time a paper from Mt. Wilson has been rejected. This must not happen, the editors should be forced to accept it.

Weart:

Sent a petition around to whom?

Chandrasekhar:

To the members of the staff, at Mt. Wilson, to sign it. Bowen heard of it. He tore up the petition. He said, "We don't want to be treated differently from the others." It was a long struggle.

Weart:

What about Harvard?

Chandrasekhar:

The same thing.

Weart:

Did they start submitting papers?

Chandrasekhar:

In time, they had to.

Weart:

But for a while they held back?

Chandrasekhar:

Yes.

Weart:

This raises a lot of questions that I didn't ask for the period of the thirties, but I think they apply to the whole period up through the early fifties, and that's about the relations among Chicago, Lick, Mt.

Wilson, Harvard and so forth. Maybe to start, considering not just the APJ but the others, one thing that I wondered: were the bad relations between Chicago and Harvard, did they have something to do with good relations between Chicago and the California observatories?

Chandrasekhar:

In part. There was intense rivalry, you know, among the astronomers. I was not a part of it, because as I told you before, till the late forties — when I felt sufficiently secure in myself and felt much more comfortable as an integral part of the U.S. astronomical community, with equal responsibilities with the rest, that I had a share in them and that I had just as much voice in it as anybody else — that attitude came to me only in the late forties; and this coincided with my taking over the JOURNAL. I always felt that the rivalry between the different institutions was terribly bad. Have you ever thought of the fact that Hubble was never nominated for the presidency of the American Astronomical Society?

Weart:

No, I hadn't. I'd just assumed that he

Chandrasekhar:

— the Eastern Establishment was against it. And you know, it's a very interesting thing — there are some people who never become a part of the

Establishment. I am not a part of one. [Tape # 3 (Side 6)]

Weart:

You said you were not part of the Establishment in one of your letters to us. I have to ask, surely you're part of the Establishment, having been editor of APJ?

Chandrasekhar:

Do you know how many times people tried to impeach me during my editorship?

Weart:

No.

Chandrasekhar:

In fact, two years before I gave up the editorship, Arp I used to reject Arp's papers outright, several of them. Or I would say that he must cut out all the theoretical parts, and publish only the observational parts. He started an impeachment. He went to the various directors, and wrote letters to the editorial Council. The editorial board asked me what my position was? I said, "I refuse to participate in the

discussion. You can do what you like. I won't be a part of it." I don't want to go into the long story as to finally how many years it took me to get the JOURNAL back to financial stability. Let me only say that when I finally gave up the JOURNAL, I arranged that the University transfer to the Society half a million dollars, which was the reserve fund I had accumulated. And in 1960, or even already in the late fifties, I eliminated the special privileges of the ASTROPHYSICAL JOURNAL with respect to the press, in not paying overhead. I started paying overhead, because I said the JOURNAL had to be independent. An enormous difficulty. It was enormously useful for me that Martin Schwarzschild who, together with me, in 1951 and '52, put the JOURNAL into the Society, helped me as the president of the Society in 1971 to essentially re-accept the JOURNAL, entirely on their behalf. There were enormous difficulties there, connected with it. You asked me some other questions — about the Establishment. Well, do you know how many people, Merrill, and all the others, thought that the ASTROPHYSICAL JOURNAL was going absolutely to shreds, because I was the managing editor?

Weart:

On what grounds?

Chandrasekhar:

Because, "He's a theoretician. He doesn't understand astronomy."

Weart:

What did these opponents feel was wrong about the balance of articles?

Chandrasekhar:

Well, that changes will take place is obvious — one can see retrospectively that they were not all advised, but they didn't know it before. And even in the end —

Weart:

No, I mean, how would they have changed it? What would they have done?

Chandrasekhar:

I don't know. It's very difficult to retrace history. But they simply felt that it was wrong.

Weart:

They thought it should be more theoretical?

Chandrasekhar:

No! More observational. I was a man who was supposedly not sensitive to the observational currents of astronomy.

Weart:

Because you kept putting in these long theoretical articles —

Chandrasekhar:

Yes.

Weart:

— and putting in your own?

Chandrasekhar:

On the other hand, after I became the editor of the JOURNAL, I never published in the JOURNAL for ten years.

Weart:

Oh, that's right. And then after that you began to publish very regularly in the JOURNAL.

Chandrasekhar:

Yes. For example, I have received letters like this: "Who referees your papers?" Always from somebody whose paper had been rejected: "I see that the last issue contained a paper by you. Who refereed it?" I told him, "You do not know who referees of your papers are. The acceptance or rejection of it is an editorial judgement, and it was the same editorial judgement that this paper of mine should be published." Well, people don't like those things.

Weart:

I see. Have you, in fact, had any of your papers refereed?"

Chandrasekhar:

If I were the editor, I would answer by saying, "I won't answer that question." But I answer you now: I always referee my papers, privately. For example, I

send papers to the Royal Society now. I haven't published in the JOURNAL for the last few years. I published regularly in the Royal Society. They always referee papers submitted to them: but I have them refereed first privately.

Weart:

By someone around here?

Chandrasekhar:

Yes. I have never sent a paper in my life to any journal, without it having been refereed by a person whom I consider competent. Never.

Weart:

I see. And there's enough people around Chicago that you can —

Chandrasekhar:

— or outside; I send it outside. And when I write to a person to ask for comments, it isn't true that a letter comes back saying, "It's fine." There are quite long reports and I incorporate them in my papers.

Weart:

Let me ask you — has there been a serious attempt) perhaps at the beginning or since, to split the APJ off from the University of Chicago entriely?

Chandrasekhar:

It shows how difficult it is. One of the conditions I made for relinquishing the JOURNAL, was the the JOURNAL should continue to be published at the University of Chicago Press, for five years after left, or three years. I made that stipulation. I had the great good fortune that Schwarzschild was the president [of the AAS]. I'll tell you the kind of problems that arise. There was an agreement to be signed. And of course, I wrote the agreement.

Weart:

A contract renewal.

Chandrasekhar:

I had to write the contract. Don Osterbrock was on the editorial board. He read it and said, "Chandra, that is out of the question." A copy went to Martin Schwarzschild. Martin Schwarzschild very agrily

called me on the telephone and said, "Chandra, but this is not what Levi* said!" * Edward Levi, The Univ. of Chicago Present. I told him, "Martin, what Levi told you is what I had asked him to tell you. What you're reading is not what Levi wrote, but I wrote it. I don't see any contradiction. Now, you think it over, and you suggest changes, and I will make all the changes you want." Next morning, he called up and said, "Chandra, it's OK. The point is, there's so much emotion in these matters. And I knew that if I can convince Martin, Martin can convince the others. He's a tremendously popular person. I could not have got the contract through within the Society and the University, except for the fact that Martin Schwarzschild was the president, and I arranged my time of resignation to coincide with that time.

Weart:

Ah, I see.

Chandrasekhar:

I tend to become intolerant when something which seems to me obvious is not accepted by somebody else. Because I try awfully hard to be as fair as

possible. I write something, and somebody else brings up some point which seems to me irrelevant and absurd — I become impatient. And to become impatient in discussions among equals is not the way to get things done. Whereas, I know that with Martin I can talk, and I know that if I convince Martin, then I am sure that I am fair. And Martin can withstand my impatience perfectly, you see. [Laughter]

Weart:

Yes. How did you get to know him? Was this when you went to Princeton around 1940?

Chandrasekhar:

No, Martin - when he was at Harvard in the late thirties —

Weart:

'36 or '37 —

Chandrasekhar:

— yes, and he came to Chicago and became a very good friend of mine, you see. We have been very good friends all the time.

Weart:

Then you saw him again at Aberdeen. Have you gotten down to Princeton fairly regularly since then?

Chandrasekhar:

We arrange to meet every year. Usually when I go to Princeton, I go on a Saturday afternoon, stay with him on Sunday, and don't go to the observatory. I make visits to Princeton incognito in the sense that I don't want to go and meet the rest of the people there. We have been extremely good friends. We see each other at least once a year. We talk on the telephone socially quite often. Every three months, either he calls us or we call him. Like last night, we called him - he's going to Germany this morning; I didn't know that.

Weart:

I didn't know it. How long will he be there?

Chandrasekhar:

Three days.

Weart:

Well, have we finished with the APJ I don't know too much about what was going on, so perhaps I can't ask you the right questions. I am curious about what your relation with the editorial board was like, or I should say, with the editorial boards.

Chandrasekhar:

I can answer that question. I had absolutely no difficulty with the editorial board. I used to go to the Council meetings, ask for page charges increases, ask for subscription increases, nominate people to the editorial board, with absolutely no hesitancy. Absolutely none. All the fears which people expressed at Chicago, as to what was going to happen to the JOURNAL, were completely and totally lost. I had marvelous relations with people, and the editorial board cooperated with me all the time. But of course, up to a point I selected my own friends.

Weart:

The board then served as a shield for you against these attempts to have you ousted?

Chandrasekhar:

It never went up to the editorial board, except very much towards the end. And there I simply refused — I said, "I can tell you my procedures, but I can't tell you what I do in given instances."

Weart:

To what extent has the editorial board played a role in determining the balance of the JOURNAL, the types of articles?

Chandrasekhar:

I was the complete master. In fact, I don't think there has ever been an editor more totally responsible for the Journal or more autocratic. For example, take the "Letters" which I started. I refereed all the Letters. No letter was published which did not approve; letters were not refereed outside.

Weart:

Every Letter?

Chandrasekhar:

Every single Letter during my time. I refereed it myself. And everyone knew that its rejection depended upon me, not upon anybody else.

Weart:

Well, it's often happened, a journal has been under an autocratic editor — if you look at some of the early German physics and astronomy journals, you'll find an autocratic editor — but it astonishes me that it should have been physically possible.

Chandrasekhar:

I will tell you one thing which may be of interest. You know, I have developed complete and total neutrality over the long years I spent on the JOURNAL. I have no sense of accomplishment in it. And I have no sense of having done anything beyond what happened to be the things I had to do because of circumstances. I mean, people ask me, do I miss the JOURNAL? I don't. Do you feel relieved? I don't. In fact, it astonishes me that I kept the JOURNAL for so long. The only thing I know is, that to a very large extent, it frustrated many of the

things which I would have done otherwise, during the period. Don't forget that I became editor of the JOURNAL when I was 41, and I gave it up when I was 61. That is a period in which people ride on their reputations. But I simply had to do it 100 percent of the time. The funny thing is that when I finally gave up the JOURNAL, it was felt, by some people anyway, that I was pushed out by the University from there, and so I had a lot of job offers.

Weart:

Outside, to other places?

Chandrasekhar:

That's right. Because they thought that I couldn't possibly have relinquished the JOURNAL on my own initiative, with no prodding; people thought that I must have been forced to give it by the University, due to some misunderstandings, and that I had fallen out with the University.

Weart:

I see. Why did you, in fact?

Chandrasekhar:

In fact it is the opposite. I tried awfully hard — for example, I told you that we had a reserve fund of close to a half a million dollars. It was the University's funds, which had to be transferred. You can't do that without the Board of Trustees' approval. And Edward Levi was opposed at that time. I went and talked to him, and he agreed to go along. And then he told me, "Chandra, the *ASTROPHYSICAL JOURNAL* is one of the goodies of the University. Why do you want to give it away?" I said, "So long as you feel that the *JOURNAL* can be run here at Chicago, worthy of the way in which the University should run it, we should keep it. If I fail — and if you cannot supply me with an alternative to the way in which it has been run — isn't it time that we gave it up, so that the *JOURNAL* does not get destroyed?" Then Levi told me, "It seems to me that if you have loyalties to the University and to the *JOURNAL*, the *JOURNAL* always wins." I asked Levi, "Would you have it any other way?" He said, "No." My relations with the University, with the editorial board, and most of all with the press, the compositors, — it's one of those marvelous occasions that the employees of the press,

compositors) proofreaders, they had a dinner for me when I gave up the JOURNAL. And one of them made a speech: "I only set papers. I often see Chandra's Limit mentioned there. I don't believe such a limit exists." [Laughter]

Weart:

Why did you give up the JOURNAL?

Chandrasekhar:

Well, after all, I had kept it for 20 years. That's the first thing. And the second thing is that, literally, if I had died one year before the JOURNAL changed hands, nobody would have known what to do with it. It was simply not fair that a JOURNAL, which had acquired the national prestige that it had, should be so fragile in its structure. I mean, a national journal should have a national responsibility. I come back to the beginning, that it exactly what Shapley said, a national journal just have a national responsibility. But you do not create a national journal out of ashes.

Weart:

I understand.

Chandrasekhar:

And therefore, Shapley's remark that a national journal should be nationally sponsored is entirely right. The only thing is, he was 20 years too early in his statement. I came to his view, that the JOURNAL had to go outside the University.

Weart:

But you felt that first it had to be established.

Chandrasekhar:

Yes.

Weart:

It must have taken up a lot of your time. The growth, of course, is very striking, when you look at it on the shelf. In 1966, you went on a monthly schedule. In 1967 you started the Letters. You did take on a production manager in 1969. Did you feel an increasing drain on your time?

Chandrasekhar:

Well, take for example the production manager. Why did I set it up? Because I knew that it couldn't

change hands. That office down there did everything; we read all the proofs, we made all the advertisements; we made all the page charges; we made all the budget. Can you imagine an editor coming after me who would want to do all that?

Weart:

I should think it would have taken up all of your time. don't see, how you produced anything.

Chandrasekhar:

I had a girl, Jeanette Burnett, and I asked her to set up the production office. I had a promise from her, that she would keep the job for at least one year after I left. I set up the office in the press one year before I left, so that I could see its functioning. And this office had to be set up at certain point fairly high in the hierarchy of the press. They wouldn't have put Jeanette at that position, but I said, "I want to be in charge," and the press couldn't refuse me. So I temporarily occupied a position which was filled by my assistant. And when I left, she had to be there. It takes time, you know.

Weart:

Yes, it's very delicate.

Chandrasekhar:

Yes. That is why the giving up of the JOURNAL was a long process. It took me a total of four years to arrange the transition.

Weart:

And now you're free of it.

Chandrasekhar:

Yes.

Weart:

There are some other events in the JOURNAL. One is introducing the Letters in 1967. How did that come about?

Chandrasekhar:

Well, do you know what all the astronomers told me when I did it?

Weart:

No.

Chandrasekhar:

"You are trying to ape the physicists."

Weart:

This was your idea?

Chandrasekhar:

Yes.

Weart:

There had been no pressure from the astronomers to do this?

Chandrasekhar:

No, on the contrary. I announced the formation of the Letters at the meeting of the Astronomical Society, in Madison, I remember. The common things I heard was at lunch tables and so on, when people didn't know I was there, "Well, Chandra just wants to imitate the physicists. That is his weakness, he wants to do everything the physicists do."

Weart:

Why in fact did you want to do it?

Chandrasekhar:

It was quite clear to me that discoveries in astronomy, like Maarten Schmidt's work and quasars were being made with increasing frequency. X-rays sources were coming up. I could see that important discoveries were made, and quite apart from what the authors thought or not, they should be rapidly disseminated among the astronomical community; I knew the need was there, and —

Weart:

did you feel that they were being disseminated outside the JOURNAL?

Chandrasekhar:

No, I simply felt that the JOURNAL was here to serve, and it was not doing its duty well if it did not foresee the responsibilities which would follow. It would have been a terrible difficulty to establish the Letters in '70 or '71. At least, I felt that it would have

been difficult. I thought it was better to start it at a time when the pressure was not very great.

Weart:

Why? I don't understand.

Chandrasekhar:

Because I could see the momentum of discoveries increasing, I felt that the need will come, and it was better for the JOURNAL to foresee the need than to be forced to improvise some solution, when the need was being pressed from the outside instead of from the inside.

Weart:

I see.

Chandrasekhar:

I simply felt that the JOURNAL was there to serve the astronomers. I don't know whether the rest of the community thought so from the way I was running the JOURNAL but I tried to be desperately fair, I always used to ask myself, "am I rejecting this paper because I'm prejudiced against the author? or am I accepting a paper because I am partial to the

author?" That's a question I always asked myself. If there was any doubt that prejudice is involved, I allowed the decision to be in favor of the author. And very often, the decisions were just mine.

Weart:

I'm wondering what determined the composition of the JOURNAL? There were a couple of places where you intervened, for example, the editorial of 1959 on publication of papers in radio- astronomy. Why did you publish that editorial?

Chandrasekhar:

Because some radio astronomers asked me, "Is this paper all right?" I said, "Why isn't it all right?" So I was getting a little worried. I always used to tell them, "Anything which is good astronomy is good enough for the ASTROPHYSICAL JOURNAL.

Anything which is bad, even in conventional parts of astronomy, is not good for the JOURNAL."

Several of the people who were doing radio astronomy asked me, "Can we publish our papers in your JOURNAL?" I said, "Why not?"

Weart:

Did you ever have this come up with other fields?
Were there fields where you tried to encourage people, perhaps just by talking with them, to publish in the *ASTROPHYSICAL JOURNAL*?

Chandrasekhar:

The only. thing I ever did, and the way I would have done that is, for example; general relativity was never present in the *JOURNAL*. I started publishing in the *JOURNAL* on relativity. Others followed, you see.

Weart:

So you encouraged it by example.

Chandrasekhar:

Yes, in that sense. I think that's the one instance in which I made the innovation. Of course, people might say I did it because I was working in it, but the real reason why I did it was whether one believes it or not (what I. say) I. felt that general relativity: had now come to play a role in astronomy, and it ought to be represented.

Weart:

Did you have any difficulties in your relations with the ASTRONOMICAL JOURNAL?

Chandrasekhar:

None whatever.

Weart:

How did you decide the dividing line?

Chandrasekhar:

I never decided it . I considered every paper that came to JOURNAL.

Weart:

I see — so it was essentially up to the authors to decide which journal to send them to?

Chandrasekhar:

That's right.

Weart:

If it was a good paper you wouldn't say, "However, this would belong in AJ."?

Chandrasekhar:

There were some efforts on the part of the ASTRONOMICAL JOURNAL editors, particularly Clemence who thought that I ought to direct some of the papers from the JOURNAL to the ASTRONOMICAL JOURNAL. I told them that it was too invidious job for me to try to do it.

Weart:

Did they direct some papers your way?

Chandrasekhar:

No.

Weart:

That's interesting. So it was really entirely up to the community.

Chandrasekhar:

Yes.

Weart:

Would it be the same with, for example, SOLAR PHYSICS?

Chandrasekhar:

I never would have redirected any paper to any JOURNAL. Its acceptance was solely the criterion of whether it was good astronomy.

Weart:

I see. If you rejected it, it might be perfectly good physics, perhaps —

Chandrasekhar:

Yes.

Weart:

Well, are there other things I should ask you about the ASTROPHYSICAL JOURNAL? You know much more about it than I do.

Chandrasekhar:

I'll tell you one thing. In 1953, just about a year after I'd taken the editorship, I was working with Fermi. Our plan was, I used to meet him every Thursday morning at 10 o'clock, and we used to work till about 12, and at 12 we'd go to lunch together. Next week I would come back again to go through our

discussion, having straightened out a number of loose ends. On one occasion, when I went there, I had a huge package with me. Fermi looked at it, and obviously there was a question in his eyes.

Weart:

You had this big package.

Chandrasekhar:

And Fermi had glanced at it. There was some query in his eyes, which I noticed. I told him — I remember it very well — "It's the March issue of the *ASTROPHYSICAL JOURNAL*." He turned to me and said, "Why do you do it?" I didn't reply to the question because I did not know why I was doing it; and I still do not know why I did it. [Laughter]

Weart:

I'm curious about the whole development of the journal over this period. Aside from your own influence, what do you think have been the main reasons for the very striking changes.

Chandrasekhar:

Very simple. American astronomy became very good. A journal is what the authors write. The editor doesn't solicit articles; the articles come to him. If the editor has in some way encouraged publication of good papers, promptly, efficiently, and fairly, he has done a little service, but the credit for the quality of the journal is not the editor's. It belongs to the astronomical community.

Weart:

One other question about it. If one takes a look at it physically, compares it with one of the much earlier ones, of course, aside from the much greater size there are two things that one notes. One is that articles are much shorter, and the other, that the plates are gone, except for an occasional half-tone print. What sort of reaction did you get for these changes? How did you personally feel about instituting these?

Chandrasekhar:

The changes in the plates were not made by me, because the plates before had to be hand-inserted in

every place, and sometimes a journal would have 30 plates. Take 4000 issues of the JOURNAL, and in every single one you have to hand-insert — that kind of labor is simply not available now. Whereas if you put them all the the end, or if you want to put them in the middle, not in glossy prints but something else, it can be done.

Weart:

Even at the end they're much fewer now than they used to be.

Chandrasekhar:

For example, one very distinguished astronomer said, "I won't publish in the JOURNAL if my papers are not accompanied by my plates right in the middle." I said, "I'm sorry that you feel that way. Your papers are very good and I'd like to publish your papers, but I am sure you're not insisting that I go and hand- insert your plates in 5000 copies?" Well, the astronomer sent a paper before long. These are technical problems. On the other hand, you see that feeling shows that somehow the conventional astronomer likes to have this glossy prints in the text.

Weart:

I can appreciate it. There was nothing else to be done, I see. I wondered — have you served on any other important boards or committees, that influence the way funds have been given, review committees and so forth, government — ?

Chandrasekhar:

No. I've never served on an NSF Panel, I have never served on an ONR Panel, I have never served on any committee. In some ways, probably, being the editor of the JOURNAL gave me the shield by which I could reject all other responsibilities which might or might not have come my way.

Weart:

I see. You weren't asked, people simply assumed that because you were the editor that was enough? Or were you sometimes asked?

Chandrasekhar:

I have been asked once or twice. But I didn't have much of an opportunity. Also, on the other hand,

people might have felt that I wouldn't have — I don't know.

Weart:

Did you get an impression from people that they felt you have occupied a position of great power?

Chandrasekhar:

From people whose papers have been rejected — yes.

Weart:

I see. Maybe this would be a good point for us to stop. It's almost noon. [Lunch break]

Weart:

That gives us an hour, and that will be enough to cover the main things anyway, I think. OK, we're resuming after lunch. Now we get into your work in the early fifties on turbulence, leading up eventually to the book **HYDRODYNAMICS AND HYDROMAGNETIC STABILITY**.^{*} How did you get into this, turbulence and connection and so on?

Chandrasekhar:

I can state it very briefly. First, as I told you before, I was extremely pleased with the way my work on RADIATIVE TRANSFER ended. And after I wrote that book, I felt that phase of my work was finished, and I was planning to do something different. I remember talking to van de Hulst, who had worked with me on transfer theory during the late forties, along my lines: I told him, "Well, Hank, during the past 20 years I have been working on problems which in some sense other people had formulated — Eddington Schwarzschild, Milne and people of that kind. When will we formulate problems on which people 20 years from now will start working?" So I was anxious to make a clean break with astrophysics as it was understood at that time, and go on into something different. It seemed to me that turbulence was the area which was an intersection between problems of astronomy and problems of the type of things I could do. So I started working on turbulence. Actually, I was not satisfied with my work on turbulence as it progressed during the early fifties; I was disappointed at what was coming. I had learned the subject, published a few things, and at one point I actually thought that I would go up to the

attic and bring down my old books and papers and start working on transfer theory. I was so ashamed of the thought that I said, I shouldn't do that. Then, a month or two from that time, the possibility of working on stability problems occurred to me. And here I felt that there was a whole range of problems which one could do. Even the simplest Benard problems hadn't been solved properly. So I started working on stability problem, and they grew and they grew. Later, I found that I was making a number of new predictions. So I set up a laboratory here on the campus, in which on the hydro- magnetic side, Nakagawa worked, and on the hydrodynamic side, Dave Fultz. That was a period of collaboration between the geophysicists, particularly Dave Fultz, and the group who did the experiments. I think a number of interesting things came out. During the time I was doing these problems, Billard asked me if I would write a book on it, and I agreed. Even though. I signed the contract in 1955, I was not ready to start the book till May 1959. I made up my mind that the book had to be finished by April of 1960, when * Oxford: Clarendon Press, 190 (reprinted 1968). I was going to Israel to give the Weizmann Lectures. I'd asked the editor of the

Clarendon Press to meet me at the London Airport. I did that already in July of 1959, and it was tremendously hard work. I believe that that nine months was probably among the hardest periods in my scientific work. Of course I was editing the JOURNAL all the time, and I had students; it was a tremendous effort which I put forth at that time. It is always difficult to know retrospectively whether the effort was worthwhile or not. But anyhow, at that time I did put the effort and I did complete the book as I wanted, and the editor did receive the manuscript at the London Airport, on my way to Israel.

Weart:

I'm curious about this change. You say that when you began it, you foresaw that it would eventually be important for astro-physical applications, and yet it seems that you were moving into the physics community. You were not publishing in ASTROPHYSICAL JOURNAL at this time.

Chandrasekhar:

No, that is right. But on the other hand —

Weart:

— Did you feel that you were moving into the physics community?

Chandrasekhar:

I knew that I was getting away from the mainstream of astronomy; that I knew. But on the other hand, here was a whole area here, which had to be explored. In the book I predicted a very large number of new results, new experimental results, the notion of overstability, the fact that rotation and magnetic field work opposing each other. We verified all that in the laboratory. You are right in pointing out that my initial motive in going into turbulence and hydrodynamics was hoping to lay the foundations of future theoretical astrophysics. But it didn't turn out that way. On the other hand, it was compatible with my temperament, in the sense that here was an area in which I could work profitably. Retrospectively, my judgement has been justified. That book was published in '61, and it has gone through four printings and sold some 10,000 copies; it continues to sell.

Weart:

Where did you get your support from in this work?
Was it from the physics community?

Chandrasekhar:

I got it from the ONR.* It's very interesting. I had to give a talk to the Fermi Institute, a visitors' meeting, I had made all these predictions about hydrodynamic convection, and I made a comment at the end that it would be marvelous if all this could be experimentally verified. I concluded by saying "Sam Allison tells me that he has a 30-inch cyclotron, and that he would let me have it, but I don't have the money to refashion it for my work."

Weart:

The cyclotron was to provide the magnetic field? *
Office of Naval Research 6

Chandrasekhar:

Yes. And there's a 30-inch cyclotron which was going to be discarded, because they were building a bigger one here. Sam Allison was going to dispose of it. I said, "I wish I could get hold of that magnet

and remodel it for a hydromagnetic lab." At the end of the meeting, a representative of the ONR came to me and said, "Why don't you apply to us for funds?"

Weart:

Who was that?

Chandrasekhar:

A ONR representative.

Weart:

But who?

Chandrasekhar:

A representative of the ONR. He came to me and said, "Well, do you want money to refashion the cyclotron magnet?"

Weart:

He had been just sitting in on the meeting?

Chandrasekhar:

Yes, it was a visitors' committee. And I said, "Why not?" So I applied, and Sam Allison helped. It was

an enormous encouragement. Sam was so interested in these things that he provided technical assistance for me in the Fermi Institute. We set up the hydromagnetic lab, and we did all the experiments. All the experiments which I describe in my book were done right here. In a shed on the side.

Weart:

I see. Did Samuel Allison participate in the experiments?

Chandrasekhar:

No. He used to come around and talk.

Weart:

Nakagawa did the experiments.

Chandrasekhar:

Nakagawa did the experiments, and Sam Allison, as a director of the institute — I don't know whether as a director or as a person interested in the work, or interested in me because I was his personal friend — used to come around; that was an enormous encouragement.

Weart:

I see. So you would say, "Here is this, here is that, can you verify it?"

Chandrasekhar:

Yes.

Weart:

And they would.

Chandrasekhar:

They would.

Weart:

Did their work ever affect your theory? Did they turn out something surprising, and then you had to modify your theory?

Chandrasekhar:

No, it was always a case of making the prediction and having it verified.

Weart:

I see. Well, a theorist couldn't ask for a nicer laboratory.

Chandrasekhar:

Yes. It was a very nice experience. There's one astronomical incident which occurs to me at this point. During the late fifties, when we were working on these problems, Jan Oort was visiting Yerkes. Oort came to my office one morning at about 11 o'clock and wanted to talk to me. I immediately turned around and asked him questions on some of the new things they had been finding in their 21-centimeter line work. And every now and again he would turn around and ask me about my work, but I always turned the question around and asked him something specific about this. And finally in the end he said, he said, "Now, I won't be put off, you tell me about your work." I had some pictures on my table in which we had just experimentally verified, for the first time, that the dimensions of the convection cell discontinuously change at a critical magnetic field, become very large. I had some photographs of that. So I showed it to Oort, and told

him about the phenomena. Oort turned to me and said, "Chandra, all this is fine — but when are you going to come to grips with the real problems of astronomy?" I told Oort, "Well, Professor Oort, I don't feel like painting the Madonna just yet." He was a little shocked at my remark. It does explain that at that stage in my life, I was far more interested in trying to do things which I thought were useful in science, which I was able to do, regardless of whether I was an astronomer or a physicist or a mathematician. I'm not particularly parochial about it.

Weart:

Your idea was that you would embody it in a book, then that book would be there whenever somebody needed it?

Chandrasekhar:

That's right. And the book has had influence in astronomy, I think. Perhaps not as much as the other ones, but the hydrodynamic community seems to be using it.

Weart:

Now, a further question about hydrodynamics, and particularly in the context of ONR support and so forth. Was there any feeling, either at ONR or anywhere, that this work might have military implications?

Chandrasekhar:

I was never informed.

Weart:

What about in general? Many of the problems that you've worked on are applicable, for example, to a fireball or a nuclear explosion, as well as to stars and so forth. Have you had any contact with the Cold War and with these developments?

Chandrasekhar:

I was with the Jason group* during the late fifties and early sixties. I was involved in the fusion program, with Los Alamos, particularly, and La Jolla. * A scientific group involved in advanced defense analysis. See articles in the NEW YORK

TIMES, 1972, and the PENTAGON PAPERS - SW.
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Weart:

In what respect, as a consultant?

Chandrasekhar:

Yes, I spent two summers at Los Alamos, maybe 1957-58, or '56-'57, two summers of three months each, and I spent a month with Marshall Rosenbluth when he was at La Jolla on these [fusion] problems. I was with Jason, and in fact in 1961, I spent the whole summer with the Jason group, when it met in New Hampshire.

Weart:

What was your role in that?

Chandrasekhar:

Well, they were interested in laser beams and things like that. I was working on some problems connected with that. I did write a few reports for them.

Weart:

So again they would present you with specific problems?

Chandrasekhar:

Yes.

Weart:

Did you have the same feelings about it that you had when you worked at Aberdeen?

Chandrasekhar:

No, I'm afraid during the sixties I was getting disillusioned, because I was not satisfied — like most people — with the Vietnam War, which was all coming up at that time. Nevertheless, many of my friends whom I respected like Marvin Goldberger especially who is a good friend of mine (who's now at Princeton) he was involved in that, and Steven Weinberg, John Freeman, former people from Chicago. They were all involved in it, and they asked me, "Won't you come along and do some work with us?" I didn't feel committed in the way in which I was during the Second World War. But on

the other hand, it seemed that if my friends considered it worthwhile, and I respected my friends, I would go along with it. But once the early sixties started, I discontinued that kind of work. Among other things, the load of my own research and the editing of the JOURNAL simply left us time for anything else.

Weart:

But while you were working on this, was the rhythm of your work — you would drop your work down here, go and spend a few months working on some specific problems, specific theoretical problems?

Chandrasekhar:

That's right. For example, when I was in Los Alamos, in the fusion project, they had specific problems in plasma physics. Well, I started working on them, collaborated with Ken Watson, and Alan Kaufman. When I was in the Jason group, Goldberger suggested one very specific problem in the propagation of electromagnetic wave, something which I felt I could do, and during that month and a half I did nothing but that..

Weart:

I see. You weren't involved in discussions, speculations as to what could be made?

Chandrasekhar:

No. They assigned a certain problem. Of course there were meetings, at which people were talking about the problems they were working on, I was in the group, I could hear what people were talking and in general we knew what was going on. But I pretty well concentrated on the particular problem that was assigned to me, and tried to do it.

Weart:

Another question about this period. I notice at one point in one of your papers you say that you and Donna Elbert made tables with the Watson Scientific Computing Lab.

Chandrasekhar:

Yes. That was a residue of the old work. In my RADIATIVE TRANSFER I had provided the exact solution for the illumination of the sky. But presenting the solution in the form of tables required

computing what I call the X and Y functions for large numbers of parameters and variables. I arranged with the Watson Laboratory. L.H. Thomas was there, and he helped me program this problem, and we did a fair amount of work there. This went on for several years. Of course, at that time, I was not actively interested in it; it was a side problem which was going on. And when the whole thing was finished, we assembled it all and published it in the American Philosophical Society [TRANSACTIONS].*

Weart:

What is your feeling about the role of digital computers in theoretical astrophysics and physics these days — or let's say, since the fifties?

Chandrasekhar:

I would say that in the hands of good people it's a marvelous tool. But it also enables second-rate people occasionally to do first-rate work.

Weart:

How is that you have rarely made much use of these machines?

Chandrasekhar:

Two reasons. As my mathematical friend Harold Davenport used to tell me years ago, "It's difficult to teach an old dog new tricks." That's one thing. The second thing is, even apart from anything else, when the sixties came along I found that I wanted to change to problems which did not require a tremendous amount of numerical work. With that intent I went into relativity.

Weart:

Why deliberately, that you didn't want to do numerical work?

Chandrasekhar:

I felt that I wanted to work on more contemplative matters. I wanted work which would stand on the merits of its theoretical implications, and not require tables.

Weart:

Was this an esthetic feeling? * Vol. 44 (1954), 643-728.

Chandrasekhar:

No, I suppose it's something to do with vanity. For example, take Dirac. There's not a single table in any of his papers. But I want you to be absolutely sure that I am not raising a hierarchy in scientific work. Heisenberg, for example, some of his early work on X-ray spectra and atomic spectra had required some amount of numerical work. So I think numerical work is an integral part of scientific work. But it is also true that there are types of theoretical work which will stand on its feet, simply on the strength of what it is. I thought that I had been, for 30 years at that time, involved in specific calculations, specific problems, enlarging domains by solving a whole variety of problems, a range of problems, and putting them all together. Now I wanted to change the style of my work.

Weart:

I see. You knew von Neumann. Did he ever try to get you interested in computers?

Chandrasekhar:

Oh yes.

Weart:

How so?

Chandrasekhar:

Well, he was very much interested in computing, and he was also extremely interested in computing stellar models, and stellar evolution. He started talking to me about it, but later found that in his own colleague, Martin Schwarzschild, he could make much greater headway, because Martin was actively interested in those; and I was not.

Weart:

I see. And you simply were not.

Chandrasekhar:

Well, not that I would not, but the fact that at that particular time, it was not in the mainstream of my interests.

Weart:

Now, another collaborator that you had, in 1952-1953, was Fermi, and you did these two well-known papers on the magnetic fields in spiral arms and on

gravitational stability in the presence of a magnetic field.* How did that come about? How did Fermi get interested in this and how did you come to work with him?

Chandrasekhar:

It was really a part of Fermi's way of learning things. The collaboration was initiated by him, not by me. I used to see him now and again, as a colleague in the University, and one day he simply came and asked me, "Chandra, why don't we talk some problems in astrophysics, hydrodynamics and hydromagnetics, and perhaps something will come out?" He said, "Of course you'll have to teach me."

Weart:

This was at the time when you were just getting started in hydrodynamics? You had already started

Chandrasekhar:

— started on it, yes. I had in fact given a talk on the Benard convection problem so he knew that I was interested on these questions at a physics colloquium. So he wanted to talk to me about it. I

thought it was a marvelous opportunity to get to know a * APJ. 118 (1953), 113-15, 116-41. great physicist; and so I planned to meet him every week and discuss various problems. We used to talk for some two hours; and I used to go home and collect what we had talked about arrange them in order, and if anything needed to be done, I used to do it. Then next week, I would tell him what had happened to the ideas we had discussed, the previous week, We used to discuss it more. We went on for six months like that, and at the end of the period, Fermi said, "Well, we seem to have a number of new results, shouldn't we put them all together?" I said "Yes," and I undertook to write the papers.

Weart:

Why did he come into this field?

Chandrasekhar:

Fermi was incredibly curious. He wanted to learn everything that was going on. And his principal way of learning and getting acquainted with a new subject is not to read books or read papers, but talk to people who, he thinks, know something about it. I once wrote that Fermi seemed to me like a master

musician who, when presented with a new piece of music, would play on sight with great conviction. Of course, he could do it because he had a store of knowledge which he could bring to bear on any the subject. And Fermi had such a marvelous understanding of physics; that anything which was physics, when it was presented to him, he knew how to solve.

Weart:

I'm interested because this is not physics anymore, it's astrophysics, and I've always been interested, for example — you know, the Fermi cosmic ray mechanism and so forth.

Chandrasekhar:

It started in discussions connected with all these matters, yes.

Weart:

So then he went on and did it after these talks about

Chandrasekhar:

Yes.

Weart:

You don't know why he decided to pick this as something to look into, rather than some other field of physics?

Chandrasekhar:

Of course it is difficult for me to speculate. But I can imagine that after all, Fermi at that time was 50 years old. He had been in physics for 30 years. Clearly, he had a period of work in Fermi statistics, theory of beta decay, neutron reactions, the reactors in the war; then with the pions in the Chicago Cyclotron - it is natural that someone like him might say, "Well, I want to change my area of interest."

Weart:

You see the main thrust here, one of the things that people have pointed out as very remarkable about astrophysics, particularly since the war, is the way that astronomers and physicists have gotten mixed in with each other. This seems to be one example; I'm sure you know of others. I'm interested in the mechanism, and why physicists would start to find

astronomers, astrophysicists, interesting — even before general relativity came along?

Chandrasekhar:

By and large, I suppose that it was due to the simple recognition by the aware physicists that astrophysics provided a realm where principles of physics could be used, principles of physics with which astronomers were not familiar then. It was really a case of many distinguished physicists going into astronomy, rather than the reverse.

Weart:

Yes.

Chandrasekhar:

And indeed, there was a considerable resentment on the part of the astronomical community, at the physicists coming in.

Weart:

Taking over their territory?

Chandrasekhar:

Yes. That feeling was there.

Weart:

Is that still there?

Chandrasekhar:

Well, to give a minor example, I was in one of the selection committees for the National Academy for the membership during 1973 & 1974 and the people who had been elected to astronomy the previous years had all been people who were not professional astronomers, but coming in from the outside like like Giacconi and others. The astronomy member there resented the fact that no professional astronomer had been elected. I was also representing astronomy at that time, and the other thought that I was a 'traitor' in saying that I didn't consider there was any difference between the two classes.

Weart:

Why do you suppose it is that the physicists have been able to come in so strongly?

Chandrasekhar:

I won't say physicists as physicists. There are some physicists whose perception and understanding,

whose breadth of knowledge, convinced them that the principles of physics have a role to play in astrophysics. Take Charlie Townes. After all, he was very good in spectroscopy; he was very good in these transitions which come in molecular lines. He had been a physicist working with lasers and masers for years. He looked around, he saw the discovery of formaldehyde [in stellar space], the discovery of OH masers — said, "My God, if I can understand the universe by applying my ideas, why shouldn't I go there?" I don't think one simply should say, here is a case of one group of people being superior to another. It's rather that there have been physicists so aware of the significance of physics for the understanding of astrophysical phenomena that when that knowledge is wide, and they come across astronomical phenomena in which what they have been doing has a bearing, they naturally go into that. Townes could have done more in laser spectroscopy — but now, he has infinitely more fun. So why would he not do it?

Weart:

I agree. I've always found it attractive myself. To get back a little to your own work and the relation

between physics and astronomy — in '61 you came out with HYDRONAMIC AND HYDROMAGNETIC STABILITY, which I suppose still is as much used by physicists as by astronomers. You would know better than I.

Chandrasekhar:

I think it is used more by the hydrodynamics people. You take the JOURNAL OF FLUID MECHANICS, almost every issue has reference to my book, I think.

Weart:

Right. Now, in his report to Yerkes Observatory for that year, Morgan drew attention to what it might do for astronomers, and particularly the implications for pulsars, and that led me to wonder whether in fact the discovery of pulsars had any effect on your work? Did it have an impact on you?

Chandrasekhar:

I wouldn't say that it had an impact on me any different from any aware person at that time. Of course the pulsar discovery was in the late sixties.

Weart:

Right, of course, Morgan was not in fact referring to pulsar work at that time, referring to rotating small magnetic stars; of course I had the wrong period. Still, when pulsars came along, did you have any wish to go back and do some of this hydromagnetics?

Chandrasekhar:

Not that. But I was in many ways pleased that in the early sixties when I seriously started relativity, I worked out, for the first time I think, the stability of radial pulsations of relativistic stars and discovered an instability; I showed in particular that a white dwarf, before [reaching] its limit, becomes unstable.* I thought there was some poetic justice in my returning to my first love and showing that the limit really did not exist, in the sense that before the limit was reached, the star became unstable; that nature had a way of avoiding the singularity. I was pleased with that.

Weart:

Did you think at that time that that meant that there were no such singularities?

Chandrasekhar:

No, I only said that the singularity does not occur at the white dwarf stage. And of course, one didn't want the singularity to occur at the white dwarf stage. One wanted it to occur at the Schwarzschild radius.

Weart:

I see. At what point did you begin to think that the singularity might actually occur? Not just the limit; through your early work, you simply said that the star somehow avoids going into the singularity. At what point did you begin to think that in fact singularities might exist?

Chandrasekhar:

I would say that I was pretty convinced of it by the early sixties, myself.

Weart:

This was because of the discovery of quasars?

Chandrasekhar:

No. Because I had got back into relativity. And began to think seriously of these problems once again. To me, somehow, certainly in 1964, when I did the relativistic instability, it was fully * APJ 139 (1964), 1396-98 (with R.F. Tooper); 140 (1964), 417-33. PHYSICAL REVIEW LETTERS 12 (1964), 114-16, 437-38; see also. in my mind that the relativistic instability would make the neutron stars unstable, if the masses exceeded a certain limit; and that a black hole must form, in supernovae explosions if the residue left is outside range of stable neutron stars.

Weart:

But now instability might simply mean that no Black Hole forms, that simply it's all blown out.

Chandrasekhar:

Well, it is very unlikely, that if you have a star of ten solar masses, it will eject precisely nine solar masses. It would seem unlikely.

Weart:

Let me ask you why you started your work on general relativity? Again, from the Yerkes Report in AJ, you say you "embarked on an investigation of the basic concepts of general relativity; "your" approach was from the point of view that what is required is more an explanation of what is possible in general relativity, rather than what can be explained in its terms. "Then you go on to say you already had some preliminary results. This explains your attitude, which sounds similar to what your attitude had been previously also to your work. But it doesn't say why general relativity.

Chandrasekhar:

Well, precisely this. In 1930 when I first came to Cambridge, the first term in Cambridge, I heard Eddington's lectures on relativity. And Dirac told me that I ought to do relativity. But at that time my equipment prevented me from doing it. At a later time when I could have done it, I realized that

people who had worked on relativity had essentially gone in directions which were not profitable, and I was afraid that it might not be conducive to my scientific productivity to go into relativity. In 1961, when I finished my book on stability, I was not happy in the way I was when I finished my **RADIATIVE TRANSFER**. I felt that I had spent ten years working very hard on matters which were, in some sense, "small". Perhaps that's not the way to describe it. Somehow I felt that the results I had were not commensurate with the effort I had put in. I wanted to leave the area. I thought of other things. I thought perhaps of going back to pure astrophysics as before, but I was reluctant to do that. I said, why not try my hand at relativity? I was skeptical of doing it. Just as a preliminary, in 1962, there was the general relativity meeting in Warsaw; I went to that largely to see what people were doing in relativity. Hermann Bondi was there, and he gave his work on gravitational waves, and I had long conversations with Leopold Infeld, whom I used to know in Cambridge in the thirties. And it seemed to me at that time that possibly someone with my background in astrophysics could formulate problems in relativity whose solution might be useful.

Weart:

How did you feel about the field at the time, when you went to the conference? Did it strike you that the field was changing. beginning to go?

Chandrasekhar:

I thought that Bondi's work was marvelous.

Weart:

But in general, aside from that?

Chandrasekhar:

I felt that there were new directions in relativity, primarily from two directions. One was Bondi's work, which actually later led to all this marvelous work of Roger Penrose and others, you know. And then there was this work on post-Newtonian approximations by Infeld, which essentially had got into an impasse, because they hadn't really solved the problem. So I felt that going into relativity, particularly in the post-Newtonian developments, would be the right thing for me, because this was a way in which I could learn the subject, get familiar with the concepts. So right from the outset, the

principal thing I wanted to work on was to complete the post-Newtonian scheme.

Weart:

I see, because you saw this as a good way to become familiar with the entire —

Chandrasekhar:

The entire area. And on the whole, I consider myself very fortunate in having made up my mind to do relativity. Among other things, for the first time — it may be immodest to say so — for the first time, certainly after the early forties, I felt I was working in an area in which others were working in many ways were far more equipped than I was. I felt that I had a chance of being in close scientific proximity with people of the highest caliber. Certainly, to have known well and consider among my friend people like Roger Penrose, Stephen Hawking and Brandon Carter — it's a marvelous experience. It's a kind of intellectual stimulation which I had in fact not had before. Of course, I worked with Fermi, Fermi is a great physicist, but it was simple problems on which he worked. But here Im now in a community of young brilliant men. I was in Cal Tech for my first

sabbatical in 1971. There was Bill Press, there was Sol Tukolsky, and every afternoon they used to come to my office and say, let's go for lunch together. I felt once more rejuvenated, once again with young people, tremendously bright, tremendously exciting.

Weart:

Do you exchange correspondence with them?

Chandrasekhar:

I see Sol Tukolsky and Bill Press quite often. In fact, Bill Press has asked me to come and spend some time with him at Harvard. Steven Hawking was here visiting in 1975, when he had a symposium for me for my 65th birthday. He asked me if I wouldn't spend some time with him in Cambridge [England] the following year. I told him, "Well, if that is for mathematics of relativity, I haven't done very much of that." And Hawking sort of laughed and said, "We'll change the name of the symposium." You know, its wonderfully nice to have such nice relations with young people. As you know, Stephen Hawking is one of the very best minds working in the area. And certainly among the others we must

count Kip Thorne, James Bardeen and Penrose of course is at the pinnacle — Even though in age I am very much older than these people, it has always been a satisfaction to me that these people treat me as their equal. We have marvelous times. I think I did the right thing, in changing to relativity.

Weart:

I see. Let me ask you some questions about one specific paper, that may be the most interesting for the sixties, and that's the one you mentioned earlier on the dynamical instability of large masses. I'm wondering, in this one you note that it comes out to be about the size of the quasars.

Chandrasekhar:

Yes.

Weart:

It came out to be the right radius and so forth. Had you had the quasars in mind when you started out doing this line of work?

Chandrasekhar:

Yes, I had. There were two things which happened. First, that I was lecturing on relativity, and in a way that problem which I formulated is a very natural one for one with my background. You know, the whole theory of the internal constitution of the stars started with Eddington's interest in pulsating stars. He worked out the theory of Cepheid pulsations, and Eddington told me himself that his whole investigations of the internal constitution stars started with his interest in the period-luminosity relation of Cepheids. One of the first things Eddington did was to study the radial pulsation of some spherical stars. With this knowledge, for an astrophysicist to go into general relativity, the first problem he thinks of is radial pulsations of spherical stars. So I wanted to study that. And just at this time, there were quasars in the news. And Geoffrey Burbidge and others were talking about galactic masses of the sizes of parsecs and so forth.

Weart:

Did any of this have any effect on your decision to go into relativity?

Chandrasekhar:

No, it was subsequent to that. I had made up my mind to go into relativity in '61.

Weart:

I see. But even by '61, people were talking about peculiar galaxies and things like that.

Chandrasekhar:

Yes, but my going into relativity was not derived from observations, no.

Weart:

But your choice, as you say, was a natural choice, but it was also influenced by the knowledge that these objects existed.

Chandrasekhar:

Existed, yes.

Weart:

What do you think now about quasars?

Chandrasekhar:

I haven't followed it. I know what people say about it, and by and large I think that the view that they are extragalactic in origin is probably right.

Weart:

The cosmological —

Chandrasekhar:

The cosmological explanation. On the other hand —

Weart:

You mentioned that you rejected many of Arp's papers.

Chandrasekhar:

I won't go into that. I don't want to contend the modern science would not allow modifications. But it is not going to require modifications because of quackery. Let me explain exactly what he says in NATURE. He says, "You take two extragalactic objects. One shows a red shift relative to the other. The one that shows the red shift is farther away. So it is younger. So if you take two objects of different

ages, at the same distance, the younger object will show the bigger red shift. So with two objects close together, when one is young and the other is old, the younger one must show a larger red shift!' He calls it a theorem. Now, anybody who argues like that — using such a set of English words — and I had a paper exactly like that sent to the JOURNAL. I just canceled that; "I'll publish your paper without that." It is quite all right to take observations and try to understand them. But you don't understand observations by dogmas or statements that make no sense. I was very strong in that. I probably shouldn't express myself so strongly.

Weart:

No, I can very well understand it. You're certainly not alone in your views. Do you feel that black holes — I suppose we can call them black holes — play a major role in quasars, or perhaps even in normal galaxies?

Chandrasekhar:

I think they must. I'm not very original in this. I mean, what are the theories for quasars now? They are massive stars which are in collisions, or massive

stars which collect towards the centre; but as Martin Rees has said, in all cases, you must end with one big black hole. So to say that there are enormous big black holes — by which I mean a thousand, ten thousand, a hundred thousand solar mass black holes — is natural, a common denominator for almost everything people have been saying about them. That's what Martin Rees says.

Weart:

More or less on theoretical grounds.

Chandrasekhar:

Yes.

Weart:

One further question about this 1964 paper on dynamical instability of large masses. You published it in PHYSICAL REVIEW LETTERS.

Chandrasekhar:

Yes.

Weart:

Why is that?

Chandrasekhar:

Because I thought the result was of interest and I wanted it to have rapid publications; and somehow I felt that since I was the editor of the *ASTROPHYSICAL JOURNAL* myself — and as a rule, I have always wanted my papers to be refereed —

Weart:

And you were the only referee for *APJ Letters* —

Chandrasekhar:

— for the *Letters*, and so I said, "Well, let me try if those people would except it.

Weart:

Just to see.

Chandrasekhar:

Yes. And also, I have always felt that one's papers being refereed is terribly important, and while there have been years when I could have published any of the things I wanted without refereeing, I never resorted to that.

Weart:

I understand. That only take us up to 1964 which is ten years ago, and I don't feel competent to judge or comment on the work you've done in this past ten years, or know what questions to ask. I wonder how you would characterize the work you've done in the past ten years? The main lines of it?

Chandrasekhar:

The essential thing I would say is, my work during the sixties was unfortunately divided. When the sixties started, I expected to spend all my time on relativity. But instead, I started working on the theory of ellipsoidal configurations in collaboration with Norman Lebovitz. And it grew and it grew. I felt that this is an area which had to be set straight, and Norman and I were putting it in order. That essentially meant that till about 1968, about half of my research time was going to that.

Weart:

And this was completely separate from the general relativity work. This is a whole different problem.

Chandrasekhar:

A whole different problem. And I just went on. At an earlier time it would have been marvelous, because it was also one subject which was growing by itself, and essentially without any effort on my part. The methods Norman & I had developed were adequate to solve all the problems. But at the same time, I was only half looking at it, and I was doing it more out of a sense of duty.

Weart:

How did you do this? Did you take one week on and one week off, or what?

Chandrasekhar:

I sometimes did that way, yes.

Weart:

Deliberately you set aside time to work on it.

Chandrasekhar:

Or I would spend two or three months on the ellipsoids, and then I would go and spend two or three months on relativity, and by the time I would

go to the other field it was a terrible strain. And in addition, of course, the whole matter of trying to get the JOURNAL to an end — that took me about five years; from 1966 on I was constantly preparing to give up the JOURNAL. But I would say that from 1970 on, I have tried to concentrate on relativity. I went through a period of disappointment in my work, in the early seventies. The only thing I can say about my recent work is that I got interested in the theory of the Kerr metric, largely because of my association with Bill Press and Sol Tukolsky in Cal Tech in 1971. I've been studying the properties of the Kerr metric.

Weart:

I think you mentioned earlier to me about the beauty that you see in the Kerr metric. At what point did this happen? Was this in the seventies?

Chandrasekhar:

I think the importance of the Kerr metric was clear to me in late '69, not because of anything I did, but because of Carter's work. I was looking at the subject from the outside, and admired all the things

which went on. But starting in 1974, I started working on my own.

Weart:

Does it seem to be a field which is developing by itself for you?

Chandrasekhar:

It's a lot of effort.

Weart:

To push it.

Chandrasekhar:

For example, I have been working very hard during the past one year on this one thing, and trying to finish this thing. I'm planning to write a book, you know, on the Schwarzschild and Kerr Metrics.

Weart:

No, I didn't know that. That's your next book?

Chandrasekhar:

Yes. In fact, my work on the Schwarzschild and Kerr Metrics is coming to some kind of an end now and I want to put them all together, as a book.

Wear: What will you do next?

Chandrasekhar:

Well, you know, I shall be 68. Wear: You seem very vigorous. Have you given it any thought?

Chandrasekhar:

Well, it takes a toll on oneself. I don't know.

Wear:

Let me ask then, what do you think should theorists attack over the next ten years? What are the areas that you'd like to see theorists in astrophysics in general go into, that are likely to be fruitful, in the next ten years?

Chandrasekhar:

Well, really, I couldn't say that. In fact, have rather a different kind of view. I tell all the young men that if I were they, I would be terribly discouraged, because

with so many people working on so many problems, I wouldn't know what to do. felt But seriously, I've never that I could dictate to other people, or even privately to myself. My attitude to science has always been: what can I do, within the limitations of my ability, to serve science usefully, as I see it? I never have been a missionary in any sense. So I wouldn't know what people should work on or should not work on. And when I say this, it is not any modest remark. When I say I really do not know what I would recommend, I mean exactly what I say.

Weart:

Let me ask you some more specific questions. I mentioned to you earlier that we were surveying some people to find out how they feel about current problems, and I'd like to ask you about some of these current problems also. I'd be very interested, taking you as one test point in the entire astrophysics community — for example, how do you feel about general relativity and the Friedman model? Do you feel it is an adequate framework for people who do cosmology?

Chandrasekhar:

As of now, it seems to be, yes.

Weart:

Do you believe that Einstein's formulations of the general theory of relativity is likely to be permanent for cosmology, that one needn't introduce other terms, other types of relativity?

Chandrasekhar:

I wouldn't think so. It is clear that the very earliest status of the universe, the first two or three minutes, may require modifications, but I would not offhand see why general relativity should not be adequate to account for cosmology in the large, any more than why Newtonian theory would not be sufficient to account for galactic structure. No one thinks that Newtonian theory is not adequate. And why should general relativity not be adequate for cosmology, except in the very early times?

Weart:

Have you ever had any doubts about the Big Bang? Over the last 30 years, has that been the —

Chandrasekhar:

I personally have not. But on the other hand, I am just a normal citizen, as far as that goes, in the sense that I have read it and discriminate what people say. I haven't worked in it myself,

Weart:

Right, in that sense you're simply one member of the whole community. Also, in the same context, how do you feel about the questions of whether the universe is open or closed?

Chandrasekhar:

I don't have any definite views. But on the other hand, I am slightly — I am not in sympathy with those who insist that the Universe be closed. It seems to me a subject which one should let our own experience, theory and observations, eventually tell us, rather than have a predisposition on that.

Weart:

Yes. Somebody said, "Whether or not the universe is open, our minds should be."

Chandrasekhar:

Yes, exactly right. That states my position right there.

Weart:

We have just enough time, I have a few more general questions that we like to ask people, to try to get a picture as a whole human being. The first one is, do you have any strong convictions of a religious or philosophical nature?

Chandrasekhar:

No. In fact, I can characterize myself definitely as an atheist.

Weart:

How do you feel then about the universe which you've been studying all this time?

Chandrasekhar:

Truly, in that sense, the most remarkable thing for me about the universe, the astronomical universe in particular, is why it is that what the human mind conceives as beautiful finds manifestation in nature? You take the ellipses and conic sections which Appolonius wrote about. You know the enthusiasm

with which he writes about it? The incredible properties of these curves. And he talks about the beauty of these curves; he discerns them as beautiful. Who would have known that centuries later, those curves are the orbiting of the planets? How does it happen that the human mind thinks certain abstract concepts and thinks of them as beautiful? And why do they find replicas in nature? The Kerr metric is an example. Kerr discovered it in trying to explore Einstein's equations. They represent the exact descriptions of black holes in nature. It seems to me that there are a number of instances in which what the human mind perceives as beautiful has counterparts in nature is; and this to me in many ways is a very sobering thought. I don't understand that. Heisenberg had a marvelous phrase, "Shuddering before the beautiful." I would say that is the kind of feeling I have about these things.

Weart:

Is this the feeling in this photograph that you have on the wall here — a person climbing up a ladder, and at the top of the wall there's some enormous, symmetrical structure above him that he can't quite reach —

Chandrasekhar:

that's right. That is a the kind of feeling I have.

Weart:

I see. In terms of relationships with society, have you ever been concerned about the benefits that people might ultimately derive from your work, or the effects it might ultimately have?

Chandrasekhar:

I'm aware of that. But on the other hand, so much is said about the usefulness of science that I have been more concerned with the fact that people seem to completely put aside the cultural value of science. Science is a perception of the world around us. Science is a place where what you find in nature pleases you. That one can derive joy from studying and understanding science, that one can learn science the way one enjoys music or art — it seems to me people ignore these aspects. Indeed, I would feel that an appreciation of the arts in a conscious, disciplined way might help one to do science better. That's my personal view.

Weart:

Have you always felt this way? Even when you were in Madras, did you have these feelings towards science?

Chandrasekhar:

No, I couldn't say that. This is a feeling which I have towards science now.

Weart:

It's developed over the years.

Chandrasekhar:

Developed over the years, probably over the last 20 years.

Weart:

As a result of your contacts — ?

Chandrasekhar:

Essentially, my own experience in science. After all, the step between trying to do science which is useful, which is profitable, which at the end you see

as an architectural whole — the transition to what I am saying is a gradual one.

Interview Session - 3

Weart:

We did discuss a lot of specific examples, but I wanted to ask in general about your working habits. How do ideas, scientific ideas or programs, come to you? I have some specific questions about it but maybe you could just respond to that.

Chandrasekhar:

As a rule the sequence of investigations I have undertaken have followed approximately along the following lines: when I find that my interests in the area in which I am currently working are beginning to fade, I look ahead and select a general topic on which I should like to work next. The next stage is to learn the subject by reading a book or more likely a series of review articles. Often I am attracted to a particular aspect of the subject because I feel that what are known in it are incomplete and incoherent; they do not form a pattern that I find satisfactory to my taste. This dissatisfaction leads to a problem at a modest level; other problems follow, my ideas begin to clarify, and gradually a point of view emerges.

Weart:

Have you ever started into an area and found that it wasn't particularly interesting, that is, read up a bit and decide, No, that was not a fruitful field?

Chandrasekhar:

An instance of this kind occurred when I started working in the field of turbulence in the late forties and early fifties. I did publish a few papers in the subject for two or three years; but I found that I was not making much progress. Moreover, the area was becoming controversial. I therefore left the subject and went on to problems in hydrodynamic and hydromagnetic stability which occupied me all during the fifties. My work on the classical ellipsoids during the sixties started in a similar way. The subject was more susceptible to the kind of treatment which appeals to me. I found that there was a method which I had developed which gave certain of the classical results in a very simple way. I therefore started learning the subject ab initio and found that there was a whole lot in the subject which was incomplete; and some of it was plainly wrong. I also found that my method was capable of answering

all the questions that one may wish to ask in a very complete manner. I spent several years on the subject. I should add that I collaborated with Norman Lebovitz (a former student of mine) on many of the aspects.

Weart:

When you first went into it you might not have seen that it would lead so far?

Chandrasekhar:

No. It was an instance where I found that the subject began to grow on its own. And that is the way it often happens to me. For example, some three years ago I got interested in the general relativistic theory of black holes. Some major contributions had been made by others to the subject; but I did not think that the subject was organized in a systematic way: it appeared very incoherent and there seemed to be many strands that were hanging in thin air. So I started working in this area; first, in a very modest way, beginning with the Schwarzschild black hole. Soon I found that I could extend my methods to the Kerr metric. The subject seemed to grow and eventually I was able to obtain a complete solution

to the entire set of equations governing the perturbations of the Kerr black holes. My method of work has always been to start at a very modest level, and find out whether I am able to develop methods which will solve known problems in a much simpler way than had been done before; and if I am successful in finding simpler methods of solving problems, which had been considered difficult, then I begin to apply my methods to problems of increasing difficulty and scope.

Weart:

I see. So the key point may be in searching for the particular methods that are to be applied?

Chandrasekhar:

It is difficult to know which comes first. But in all cases, it begins with my dissatisfaction with the work that had been in an area. Then I try to reorganize the subject in a coherent way; and in doing so I quite often find that I am able to develop techniques of solving problems in the area which go much further than what had been accomplished before.

Weart:

I was interested in your Ryerson Lecture*. You made some comments on Newton, and one couldn't help wondering to what extent they might apply to theoretical physicists in general. For example the famous statement that Newton was "always thinking on things". Do you "always think on things", do you keep a problem in your mind day after day?

Chandrasekhar:

When I am working in a certain area, and I have been blocked by some difficulty, then I am constantly aware of the problem and explore means of overcoming the difficulty. Sometimes the difficulties are of a technical nature requiring a novel approach; and often such technical problems keep me at bay, sometimes for weeks at a stretch. My thoughts concerning a subject are generally at two levels; a relaxed contemplation of what the subject is about, its arrangement and its coherence; and a strenuous effort to solve particular technical problems. For example, in the last piece of work I have done on the complete integration of a set of 76 equations of Newman and Penrose which replace

the conventional Einstein equations, the technical problems of reductions of the equations to a manageable set is already massive.

Weart:

This general relativity stuff is amazing, yes.

Chandrasekhar:

I found that after some months of effort, I got to a dead end. I simply did not know how to proceed. I was completely blocked for three months. Then I started afresh and gradually the problem untangled itself, and the final solution was found six months later. But during those six months, I was constantly worrying over overcoming the sequence of stumbling blocks that constantly appeared on the way.

Weart:

When you say you went at it fresh, do you mean you went back to first principals, to the original problems?

Chandrasekhar:

Well, I was going along a certain direction; but since that way of proceeding was checkmated, I had to go back to the beginning and start along a different direction. The different point of view from which I started led to many alternative routes all along the way; and I always had to select one. Retrospectively, I am somewhat astonished that when there were so many alternative paths that I might have taken, the ones I did take seemed to have been the right ones. I can see that they are right now; but I did know this at that time.

Weart:

I see. Although you said sometimes you did get into a blind alley and then you'd have to back up.

Chandrasekhar:

Back up! I had proceeded cheerfully along one line; and found that at the end of three months it led to a dead end. I had no choice but to go back to the beginning and start afresh.

Weart:

I see. To what extent do you keep the physical idea in mind while you're doing this and to what extent is it a matter of mathematical formalism?

Chandrasekhar:

Well, in the particular instance that I was talking about, the problem was one of solving a large number of equations; and the choice of the sequence is a most important one. In other instances, the problems are physical; and the principal questions concern the best way of formulating them as mathematical problems.

Weart:

Do you sometimes find yourself sort of going back to the physical problem for guidance in making your way through the mathematics?

Chandrasekhar:

My concern has always been one of formulating well defined physical problems in mathematical terms. Once I have selected a physical problem for a

solution, then the principal questions revolve around how best one may formulate it.

Weart:

I see.

Chandrasekhar:

And then the problem of solving it. For example, I became interested in the polarization of the sunlit sky. The first question was how to take account of polarization in formulating the equations of transfer. In other words, the question concerns polarization itself: it is not adequate to describe it in the conventional terms of total intensity and the degree and the nature of the polarization, quantities of different dimensions. None of the extant books gave me any help. But then I found in Stokes' Collected Papers, a paper (written in 1852) which was exactly the right one for my purposes: polarized light can be represented by a vector - the Stokes' vector, as I called it. The equation of transfer becomes a matrix equation; and the problem of solving it is a major one.

Weart:

I see. So it takes place in phases.

Chandrasekhar:

Yes.

Weart:

An then, tell me -- this is something that's hard to talk about -- do you visualize things? That is, do things come to your head as words or as pictures, as pictures of mathematical symbols?

Chandrasekhar:

As a rule I find that I think concretely. There is a book by Hadamard, "The Psychology of Mathematical Invention", in which he points out that most great mathematicians think in vague and general terms, not concretely, and not in terms of formulas. I recall reading Hadamard's book in a plane; and as I was reading it, I told my wife, "I am rapidly developing an inferiority complex: I do not seem to think in the way that Hadamard says he thinks, and in the way he says that other great mathematicians also think". I think concretely in the

sense that I concentrate on particular aspects of the problem; and quite often I think in terms of symbols.

Weart:

You visualize them?

Chandrasekhar:

In some ways yes. My thoughts often concern the sequence of steps I must take towards the solution of a problem.

Weart:

Is this a sort of spoken commentary that goes through your mind?

Chandrasekhar:

Yes.

Weart:

I see. But with the symbols mixed into it.

Chandrasekhar:

Yes.

Weart:

I see. Would this vary to some extent on what problems you're working on? For example when you're working on stellar interiors or fluids might you actually visualize the interior or the fluid, or does it still take place in this more mathematical form?

Chandrasekhar:

I think on the whole I think mathematically.

Weart:

I see. Is there any sort of a tactile sense, that is any feeling of picking up these things and moving them about with your hands, that sort of an idea? Or is it more through a verbal stream?

Chandrasekhar:

It's more like a verbal stream.

Weart:

I see. That's very interesting. You know different people have very different approaches to these things.

Chandrasekhar:

Yes.

Weart:

Have you ever been guided, for example in your work on general relativity, by any more general, almost philosophical ideas or general ideas about how such things should work?

Chandrasekhar:

I do not believe that I think "philosophically". I am most often concerned with the structure of a subject and the inner relationships among its component parts. And by and large I try to formulate problems whose solutions may have some degree of permanence: their lasting interest is one which I aim at. For example, my present interest in the Kerr metric derives from the fact that it represents an exact solution for the black holes that occur in nature. Consequently, any fact about the Kerr metric can in some sense never get out of date. Both the object and what one may say about it (so long as it is relevant and it is correct) have some degree of permanence. It is the degree of permanence, and not

the current fashion, that holds the greatest attraction for me.

Weart:

I was thinking also in terms of the way you work your way through to a solution -- when you're faced with a very complex mathematical problem, whether there is anything you have to go on, other than pure mathematics itself, in order to find which of these may have the best way through. Is it simply intuition?

Chandrasekhar:

Very often it is conviction and faith that sustain me. I found for example, that what was true for the Schwarzschild black hole appeared in some sense to be also true of the Kerr black hole: there were many parallels. It then became a matter of conviction with me that every problem which is solvable in the Schwarzschild geometry must also be solvable in the Kerr geometry. And to a very large extent this conviction has been substantiated. A young man who was working with me, Steve Detweiler, did not fully share my convictions; and he left collaborating with me and went on to other things which interested

him. But kept on and now he tells me that he is surprised that my initial ideas turned out to be largely true. Of course to some extent one's convictions derive from one's experience; and I am not blaming Detweiler.

Weart:

It may go by analogy with some earlier problems you worked on.

Chandrasekhar:

Yes.

Weart:

I see. In any of this has your way of thinking changed over your career? Can you think of ways that you may approach a problem now with more experience and so forth than you used to?

Chandrasekhar:

I think that on the whole I have progressively tried to work on increasingly difficult problems. At the present time it does not make much difference to me whether I am successful. As it has been stated, it is the quest, and not the arrival, that matters. Perhaps,

to some extent, can afford the luxury of this approach. At an earlier time, the solvability of a problem was rather more in my mind. I would say that the only difference, as time has gone along, is a certain degree of confidence in pursuing more difficult problems. My attitude is, "If I don't solve the problem, well!, I don't solve the problem!"

Weart:

You separated it.

Chandrasekhar:

Well, Brian Carter told me that he did not know I was in the audience when he made this bet, you see. So I didn't get the five pounds.

Weart:

If he had known you were there he might not have....

Chandrasekhar:

That is what he said when I met him afterwards and when he knew of my separations. Well, solving problems and meeting challenges constitute some of the pleasures in doing science: someone poses a problem, and one is pleased if one can solve it. I

don't think that I have become so staid that I cannot, on occasions, get the kind of vicarious pleasure that one can sometimes find in the pursuit of science. But by and large I am generally more serious than solving a problem because someone has issued a challenge.

Weart:

I understand. Do you ever feel a certain combativeness with the problem itself? If I had worked on some problem for there months for six months I would almost take these equations as being in personal competition with me.

Chandrasekhar:

Oh yes! It is only natural that if one has already expended considerable effort on a problem, then one is reluctant to leave it unsolved. In some sense it becomes a personal struggle: the problem against yourself. And if the ultimate goals is to complete one's understanding, then one is very reluctant to accept defeat. And this feeling of personal antagonism towards a problem is a natural tendency. At least it has remained with me all my life.

Weart:

I see. It raises a question how you got that way. Can you think of anything in your training or upbringing that might have contributed to your particular approach to scientific problem?

Chandrasekhar:

With regard to what motivates one in one's scientific work, it depends upon the stage in one's career. It is natural that when one is young and ambitious, one hopes that one can achieve some reputation by making a contribution that may be comparable, perhaps, with what some of the great men of science have accomplished. But there was a point in my life, during my early years in Cambridge, when I definitely turned away from that attitude, with the resolve that, in the long run, it would be better for me to try and pursue science with the intent of contributing to it in ways which will have some abiding interest and some value. But I did not formulate this to myself as clearly in those days; but this attitude matured with the years. And it is only with this maturity, that I am able to formulate it in the way I have.

Weart:

also curious about your approach to mathematics, your ability to do this sort of very difficult problem. While you were growing up was your scientific ability recognized and encouraged in any way? Specifically scientific or mathematical ability?

Chandrasekhar:

It is difficult to say. Actually, as far as the more common scientific recognitions go, many of them came to me relatively early and rather unexpectedly in the first instance. But I cannot recall that any of these recognitions was a source of encouragement for me in any particular sense. For example, I was awarded the two astronomical medals (the Bruce Medal of the Astronomical Society of the Pacific and the Gold Medal of the Royal Astronomical Society) when I was 42; and I have been told that that was the earliest age at which anybody had received both of these medals. But the citations for neither of these medals referred to my work on the white dwarfs, I suppose because of the reluctance of the two societies in citing something which they considered controversial. So I cannot particularly associate

these early recognitions as encouraging me in the kind of work that I considered most relevant.

Weart:

I was thinking even earlier, back in India when you were first beginning. Because clearly even by the time you had left India you already had a mind which was very capable of attacking difficult scientific mathematical problems. You already had that at that point, and I curious about how they came about. For example did your Uncle Raman play any role in this?

Chandrasekhar:

No.

Weart:

Did he encourage you to go into science?

Chandrasekhar:

Not especially

Weart:

Not particularly. I see.

Chandrasekhar:

I have written a piece, which is rather controversial, You know Raman was a very controversial person.

Weart:

Yes,

Chandrasekhar:

I do not share the popular view of Raman. It should be stated, however, that I did not know him very well. After I left India in 1930 (at the age of 19), I have had the occasion of meeting Raman on only five different occasions (in 1936, in 1951, in 1961, and in 1971); and only for a few hours each time. On the other hand, I was present at his laboratory in Calcutta, in the summer of 1928, some two months after the Raman Effect had been discovered. Also, I recall meeting him in Madras at my home (in March 1928) prior to his going to Bangalore where he announced his discovery. Much of Raman's work relating to the discovery of the Raman Effect was carried out in collaboration with K.S. Krishnan. But unfortunately, Raman and Krishnan fell out in the

late forties and they became estranged. It is a very unpleasant and unfortunate story.

Weart:

But as for why you became a scientist, a mathematical scientist rather than an English literature person or whatever -- that's something that's just mysterious?

Chandrasekhar:

From the earliest time that I can recall, my interest was always in the deductive and and the mathematical aspects of the physical sciences. In a larger sense, however, I have never taken myself very seriously in the sense of having any special gifts. I never found it possible to think of my own accomplishments in science in a flattering way. I have always believed that my strength consists in making the maximum of rather modest abilities by applying myself as best as I am able. That is the way I perceive myself.

Weart:

I think that's fair enough.

Interview Session – 4

Krisciunas:

I'm here talking to Professor Chandrasekhar, first about his reminiscences about Otto Struve and then some questions about himself. You mentioned in your American Institute of Physics^[1] interview that it was at Kuiper's suggestion that Struve invited you to the University of Chicago. Had you met him before you came here to Chicago?

Chandrasekhar:

Yes, I did meet Struve before I joined the University of Chicago. Struve was on a visit to England during the summer of 1934. At that time I was a Fellow of Trinity and my room was on the third floor of Neville's Court. Struve had come to Cambridge to visit Eddington; and he had taken the occasion to call on me at my rooms in Trinity. I was not, in my rooms at that time; but he had left a note to that effect. In the note Struve expressed disappointment that he had missed me; but he left the address of the hotel in London where he was staying. I was so pleased that a person of Struve's distinction had called on me that I called him at his hotel in London

and offered to visit him the following day if that was convenient to him. Struve welcomed the offer. When I met him at his hotel he asked me to stay for dinner, at which time I met Mrs. Struve. I do not recall any scientific discussion of substance. He did, however, express interest in my work on the theory of white dwarfs at that time.

My impression of Struve at that time, which was later confirmed, was that Struve was always interested in the welfare of young scientists and that he felt the urge to make their acquaintance and perhaps encourage them in that way.

Krisciunas:

So when you came to Chicago, he already knew of you, and you mentioned that you knew of his work. Did he strike you as the same type of person once you got to Yerkes?

Chandrasekhar:

These are extremely difficult questions for me to answer. Not that my recollections of those days have faded — on the contrary, they are still very fresh in my mind — but rather that my attitude at that time to

those whom I met in the West was very different from my attitude at a later time. And indeed, retrospectively I feel that I was too "innocent" and too naive to perceive reality as it was. As I have said to my close friends, my innocence was shattered during the early fifties. I need not go into the circumstances which changed my attitude. I should therefore express a warning that my recollections of those early years and the interpretations I placed on them at that time are not the same as I would now in retrospect.

Let me preface my remarks by describing the situation as I found it at the end of my three-month visit to the United States during the winter of 1935-36. I think it was in March, 1936, that I had a letter from Struve inviting me to visit Yerkes; and informing me at the same time that he was thinking of offering me a faculty position at the University of Chicago and that I might think about that prospect in the meantime. (As I learnt later, Kuiper had first made that suggestion to Struve. At that time, Kuiper had already accepted the appointment as an Assistant Professor; and he was to join Yerkes later in the fall of 1936.) At Chicago, he had reserved a room for me at the International House. Struve drove

me from the International House to Yerkes; and I was his house guest. I have distinct recollections of the extreme cordiality of Mary Struve and Otto Struve. During my visit to Yerkes I gave a colloquium on my then recent work on the theory of White Dwarfs. But my overwhelming impression was of the personal treatment accorded to me as the house guest of a famous director.

Krisciunas:

— you felt grateful to be so wanted, in other words.

Chandrasekhar:

No, I should not say that I felt that I was wanted. I was only grateful for the treatment that was accorded me. The distinction that I make here is an important one. Struve asked me, "Would you accept a position as a Research Associate at the University of Chicago tenable at the Yerkes Observatory?" He drove me back to Chicago and took me to see President Hutchins. At the meeting Hutchins, without much ado, directly offered me the position. Even in those days, for the president of a University to interview a candidate for the position of a research associate and to offer him that position is an extraordinary

occurrence. But at that time it did not seem to me any more unnatural than to have been a house guest at the Struves!

Krisciunas:

Strüve kind of had Hutchins' right ear a lot of the time, though. They were pretty close for quite some time, weren't they?

Chandrasekhar:

I will return to your question presently; but let me continue the story of my appointment. On my return voyage to England on the Cunard liner *Berengeria*, I got a telegram from Hutchins offering me the position officially and asking for an early response. On my return to Cambridge I talked with some of my friends, Eddington and Fowler in particular. They recommended that I accept the Chicago offer and not the one from Harvard (at the Society of Fellows). And I sent a telegram to both Hutchins and Struve accepting the position.

There are some related facts that remain in my mind. After leaving Chicago and before embarking on the *Berengeria*, I visited Yale and Princeton. At both

places, Schlesinger at Yale and Russell at Princeton had me as their house guest. I was again very impressed. But let me tell you my later interpretation of why I was treated with such extraordinary cordiality by Struve, Schlesinger, and Russell: they were all afraid that I would not be able to secure reservations at hotels.

Krisciunas:

That who wouldn't let you in?

Chandrasekhar:

The hotels.

Krisciunas:

Oh yes. Yes.

Chandrasekhar:

What I mean is, I am certain now that Struve, Schlesinger, and Russell, in inviting me to be their house guest, did so, in part, because they wanted to avoid unpleasantness. I should not draw conclusions retrospectively; nevertheless, I cannot avoid feeling that the prevention of unpleasantness must have been in their minds. In fact, when I was staying with

Russell in Princeton, he strongly urged acceptance of the offer from Harvard rather than the one from Chicago with the warning, "You will find racial prejudice in Chicago; but in Harvard you will not find any. Therefore, accept the Harvard position."

Krisciunas:

Let's get back to Struve and things at Yerkes. Let me tell you a story that Franklin Roach told me. He said that when Struve became director on July 1st 1932.

Chandrasekhar:

I know that story. He's reported to have said, "From now on it is going to be different."

Krisciunas:

As I recall Roach saying it to me, he told it to me himself, he said that Struve's final line was, "Well, from now on you work for me." But maybe my recollection is — but he sounded like the type of fellow who wanted you to know that he was the boss.

Chandrasekhar:

Well, that part of Struve's conduct can be expanded. I shall refrain from it, but narrate one incident during Struve's directorship. He used to have all members of the faculty write annual reports; and then he would meet the entire faculty to discuss the reports. On this particular occasion the faculty included Gerhard Herzberg, Gerard Kuiper, Jesse Greenstein, Morgan, Hiltner, Louis Henyey and others.

Krisciunas:

This is "elephantiasis of the brain?"

Chandrasekhar:

Yes.

Krisciunas:

I don't quite understand exactly what he was saying.

Chandrasekhar:

What Struve meant was that the faculty suffered from elephantiasis of their egos and of their accomplishments.

Krisciunas:

You know, Tolstoy said once that man is a fraction for whom the numerator is what he really is, and the denominator is what he thinks he is. The bigger the denominator, the smaller the fraction, and if the denominator is equal to infinity, for whatever the value of the numerator, the fraction is equal to zero. This was pasted up in Russian on a door once. I copied it down, but I can't find it any place in Tolstoy.

Chandrasekhar:

I can imagine that Struve had remembered his Tolstoy. But where did you get this story? Who told you that? Or do I say it in the ALP interview?

Krisciunas:

It's in the ALP interview. Yes.

Chandrasekhar:

Kuiper expressed himself once, saying that, perhaps in the most critical way I have heard anyone speak of him: Struve has the bad qualities of the German and of the Russian, and the good qualities of neither.

Krisciunas:

But that doesn't really get us to what made him tick. Right? Because it's just an anecdote; it's not detailed enough on any specific questions.

Chandrasekhar:

I'm not even sure that these anecdotes represent him.

Krisciunas:

So he was the boss at Yerkes, and you were there and Kuiper was there and Morgan was there before you. And tell me a bit about how it was organized. You had staff meetings. People had to take notes and pay attention in a certain way. Were they always on the same day?

Chandrasekhar:

I was outside the mainstream of the activities; and I was treated as an outsider. I did not feel so at that time; but that was what it was. (For example, during my entire years at Yerkes, I was not trusted with the key for the dome; or asked to participate in conducting visitors on Saturdays: all the others had to participate.)

I used to attend some of the faculty meetings, hear some of the things that were going on; but most of the discussions centered on observational matters: the assignment of time at the telescopes, *et cetera*. I believe that I was not asked to attend many of those meetings. I never knew (or for that matter cared) what was going on. As far as I was concerned, I was completely and totally left to myself; and I was allowed to do what I wanted. And those circumstances suited me best.

Krisciunas:

You mentioned in your ALP interview that you never had any problems dealing with Struve. In this **History of McDonald Observatory**^[2], it's mentioned that one time, you and Strömberg I believe changed offices, and you didn't ask permission to do so, and Struve had some comments about that.

Chandrasekhar:

Let me describe precisely what happened. There were two offices, one abutting on the circular wall of the dome, and another next to it. The first one had some book shelves with glass windows; I believe it

used to be called the chemical kitchen. And the second one was a smaller office with no adequate shelves for books. Struve had assigned me the smaller of the two offices, while Strömgren was assigned the larger one. At that time Strömgren spent most of the week on the campus and visited Yerkes only for a day, or at most two days, during each week.

I had collected a moderately large library of books and journals already while I was in Cambridge; and I was anxious to have my books and periodicals clean without getting dusty. And so when my books arrived from Cambridge, I asked Strömgren whether he would mind exchanging offices with me since he was spending only one day a week at Yerkes, and the office assigned to him would be more convenient for me to have my books. He agreed.

One evening — it must have been about a fortnight after my arrival at Yerkes — I was in the larger office arranging my books. Struve passed my office while I was arranging my books and was surprised at seeing me in that office. He asked me why I was there contrary to his assignment of offices. I explained to Struve exactly what I had told

Strömngren. Struve made no comments, and left. Next morning when I came to my office, I found that the nice desk and chairs which had been in that office had been removed and replaced by an old, dilapidated table with broken drawers, and chairs to match. What is retrospectively astonishing to me, is that while I noticed the change it did not affect me in any other way! I simply took it as though it was all very natural.

That is an example of Struve's way. It also illustrates the world in which I chose to live.

The amusing thing is that I continued with the same furniture until 1944, when I was elected to the Royal Society. I remember that the day after the news of the election came, Struve came to my office and asked me whether I would wish to change the furniture in my room to better ones. I simply said, "Why not?"

Krisciunas:

OK. Well, Struve's grandfather wrote a 100 page biography of his father^[3], and I wanted to mention what the original Struve's schedule was like. He got up about 8 o'clock and had a cup of coffee. He sat at

his desk and worked from 9 a.m. to 2 p.m. Then he had some lunch. Then he took a nap for an hour and a half, and he had some more coffee. He sat at his desk from 5 o'clock till 9 o'clock, then he had dinner, then he went back to his desk for another five hours, so he worked 13 or 14 hours in the average day and did a tremendous amount of writing. I don't know when he did his observing, and I don't know when he had time to father 19 children, given his schedule, but it seems to me it was either part of the family tradition or the Germanic orientation that you worked first, then maybe if you have time you play. Is this a similar kind of level of intensity that Otto Struve had?

Chandrasekhar:

During the years I overlapped with Struve at Yerkes he was invariably the first to come to his office, probably between 7:30 and 8:00 in the morning. And he worked steadily except for short breaks for lunch and dinner. After the war, during the late forties, an English astronomer, Archibald Brown, came to Chicago with an intention to work with me. (Brown later became a professor of mathematics in Australia and I essentially lost touch with him.) But he told me

later that, on the day he arrived at Yerkes (in the evening), he was surprised to find lights in all the offices and that the Observatory was as active as during the daytime.

Krisciunas:

— was this an example that everyone followed?

Chandrasekhar:

More or less.

Krisciunas:

Either because they were really fired up, or they felt that it was somewhat expected?

Chandrasekhar:

No, it was just that it seemed to be natural to us. Kuiper always used to come back and work after dinner; and his schedule was not really very different from Struvets. And my schedule was not that different either.

Krisciunas:

When did Struve fit in his observing time? Did he give himself as much observing time at Yerkes as he wanted?

Chandrasekhar:

Oh yes. He used to observe regularly. He was very regular at it, and did more than his share.

Krisciunas:

Did he use the big refractor mostly?

Chandrasekhar:

Yes, Struve did work with the refractor. He certainly drove himself to the maximum. I believe that emotionally (I should certainly not be dogmatic here) astronomy and what happened in astronomy was closest to him. Struve's monthly articles continued in **Sky and Telescope** without intermission for many years, — is a manifestation of his interest. His articles dealt with the whole range of astronomy. It is fair to say that Struve's primary interest was astronomy. And as long as one was as

motivated towards astronomy as he was, then one would have no problem with him.

Krisciunas:

I can imagine, especially today, being in a situation where people work very hard, and saying to my boss, "I haven't spent enough time with my wife and kids lately, you know; what I'm working on is important, but I've got to go home and see them." Did that kind of thing happen at Yerkes? People would mention they wanted to spend more time doing other things?

Chandrasekhar:

I never knew anybody close enough to make that remark.

Krisciunas:

In this history of McDonald Observatory, they mentioned that in doing — reducing spectrograms and stuff like that — Struve ruined his eye convergence, which ended up to be somewhat of a disconcerting physical feature. You know, if you had to write a three sentence description, one of the

things you'd mention is that he had ruined his eyesight a bit.

Chandrasekhar:

I don't know that. It could be apocryphal. My impression always was that he was born with a very bad squint.

Krisciunas:

Oh, no, I didn't know that.

Chandrasekhar:

I should be very cautious in what I say; but I do know for a fact that he had a very bad. squint. He tried to correct it by consulting an ophthalmologist but he never really succeeded. I do not know to what extent his eyesight obstructed his spectroscopic work. I never heard him complain and I do not recall anyone telling me that Struve's eyesight had been spoiled by his measuring plates.

Krisciunas:

How tall was he?

Chandrasekhar:

Oh, he was very tall. I mean, he was much taller than — he was probably 6 feet 5, 6 feet 4, something like that.

Krisciunas:

Oh, he really was? Really?

Chandrasekhar:

I believe Strömgren was somewhat taller than he — I believe Strömgren was 6'7" — Struve was probably 6'3" or 4". There is one photograph taken in 1949 on the occasion of a meeting of the publications committee of the American Astronomy Society of which he was the president at that time. The picture includes, in the first row, Struve, Kuiper, Brouwer, Mayall, Spitzer and Paul Merrill. And he's unquestionably the tallest of the group there.

Krisciunas:

So he must have weighed over 200 pounds. He could have been a football player.

Chandrasekhar:

He could have been.

Krisciunas:

What color hair did he have?

Chandrasekhar:

Blonde. It became grey, but when I first saw him, it was blonde.

Krisciunas:

When did it turn grey?

Chandrasekhar:

I don't remember too well. It must have been in the late forties.

Krisciunas:

He was born in 1897.

Chandrasekhar:

I remember Struve's fiftieth birthday. There was a meeting at Yerkes and Joel Stebbins from Washburn happened to be visiting Yerkes. Struve took a few of

us along with Stebbins to lunch. I had told Stebbins that it was Struve's fiftieth birthday; and suggested that he should perhaps make some reference to it at lunch. Struve did not expect that anyone knew that it was his fiftieth birthday. He was both pleased and surprised at what Stebbins had said.

Krisciunas:

Can you describe his accent? His first language was Russian?

Chandrasekhar:

Yes.

Krisciunas:

Or did they speak German as well?^[4]

Chandrasekhar:

Did you happen to see the NOVA program on Kistiakovsky? My recollection is that Struve and Kistiakovsky were both students at Karkov University. They were very similar in build, very similar in their pronunciation — rather clipped, and, I suppose, slightly more German than Russian.

Krisciunas:

How familiar are you with how Struve would think of new things to work on? For example, one quote I've heard, but I don't know who said it, was that if you take a look at five stars at random, one of them is going to be peculiar, but the more I learn about Struve, it doesn't sound like him. Apparently he could look at a spectrum of any star and find something peculiar about it and think that he might want to write a paper about that star.

Chandrasekhar:

My impression is that Struve knew the spectra of the different stars he had studied almost in a personal way. For example, at colloquia dealing with his work he used to show a whole sequence of slides pointing out features in each of them as he went on. And when referring to a feature in some later slide, he would contrast that same feature in another spectrum ten slides earlier.

Krisciunas:

So he had a mind that could remember a lot of visual detail.

Chandrasekhar:

Yes! I remember once Struve telling me, "You seem to be attending my lecture very carefully. Did you find it interesting?" I told him that I was marvelling at his virtuosity and the way he could recognize the smallest detail of his spectra. I added that I was so engrossed in admiring his virtuosity that I did not follow the scientific details.

Krisciunas:

When did graduate work start building up at Yerkes?

Chandrasekhar:

The year I came to Chicago.

Krisciunas:

'37?

Chandrasekhar:

The first assignment I was given when I came to Yerkes was to prepare, together with Kuiper, a two-year graduate program.

Krisciunas:

And Struve left the organization of the graduate program up to you?

Chandrasekhar:

More or less.

Krisciunas:

So he didn't have a lot of input, he just said, "You guys take care of it."

Chandrasekhar:

Struve did not have a lot of input. For example, in this two-year graduate program that Kuiper and I had prepared, Struve was assigned two courses of lectures on stellar spectroscopy, one each year. I do not believe that he ever gave a complete course in any year. His frequent trips to McDonald prevented him from giving any sustained course.

Krisciunas:

How much time did he spend at Yerkes? A lot or half or. . ?

Chandrasekhar:

I can't be too sure, but I do know that his interruptions, going to Texas, were such that he never gave more than a third of the lectures he was supposed to give.

Krisciunas:

Who filled in for him?

Chandrasekhar:

Nobody.

Krisciunas:

Oh, they just didn't happen. H'm. I imagine he told his students to read all this, go read the journals, or.

Chandrasekhar:

More likely, he left them to examine or measure some plates.

Krisciunas:

Are you real familiar with things that he was working on back then? He was in a different realm of.

Chandrasekhar:

When I first came to Yerkes I knew that Struve had made contributions to determining the rotational velocities of stars; that he had identified the Stark broadening of hydrogen and helium lines in early stars; and that he had measured the radial velocities of O and B stars in connection with galactic rotations. His lectures on stellar spectroscopy included such material. Later, when Strömberg joined the faculty (during his first period, 1937-38), Struve built the camera which identified the H I and H II regions and which led to Strömberg's theory of these regions. Later during the war, he began his long and sustained collaboration with Pol Swings. Swings and I were good friends; and Swings used to keep me informed of what they were finding. After the war Struve became interested in contact binaries and in U-Geminorum stars. These latter studies led him to what was at that time a somewhat unconventional approach to problems of stellar evolution. He summarized all his work in his Princeton Vanuxem Lectures.

Krisciunas:

You mentioned in your AIP interview that one of your guiding principles is finding things to work on and doing the best you can and being as rigorous as you can. How rigorous was Strüve in what he was up to? It's a different type of rigor, I imagine.

Chandrasekhar:

I never had time to think about these matters. The fact is, I have never been in general sympathy with the way astronomers do their work including Struve.

Krisciunas:

Too slapdash?

Chandrasekhar:

I do not wish to express an adverse opinion. I will only say that the way most astronomers seem to do their work is not my way; it is not to my taste. But why should it be? There is no absolutely right way of doing science. The richness of science largely arises from the different ways different scientists pursue science. But to say this is not to say that I share Struve's attitude. For example, in an article

written in PASP, he concluded by saying that since astronomers of today benefitted from the observations of the astronomers of yesterday, it was the **duty** of the astronomers of today to leave a similar record of observations for the benefit of the astronomers of tomorrow. In this connection Struve made particular reference to Hertzsprung, who is supposed to have celebrated his millionth (?) setting in his program of measuring the light curves of variable stars. Struve gave other examples.

Krisciunas:

About what year was that paper?

Chandrasekhar:

It must have been in the late forties — probably 1947 or 48.

Krisciunas:

How do you do the best job at that?

Chandrasekhar:

I argued with Struve regarding his attitude. I asked him, "All scientists of today owe a great deal to Newton's Principia, and to the great volumes of

Laplace and LaGrange. We are all indebted to them — How does one discharge one's duty to them?"

Struve could not answer my question and, in fact, he got angry.

Krisciunas Do you have an answer to that?

Chandrasekhar:

The answer is, simply, don't raise to a principle the way you might have done science. As I said before, science grows by virtue of the efforts of all kinds of people with differing motives, abilities, and temperaments. Science would be poorer if all followed the same lines. In fact, I told Struve that for an observational astronomer to dictate to the astronomer of the future regarding what he should do and how he should repay his indebtedness to the past is not proper.

Krisciunas:

And to be making a list of, boy, if we only had the answers to these questions, I would be happy! You guys should work on it — making such a list is not what you would recommend.

Chandrasekhar:

My remarks should not be construed as implying that a scientist has no obligation to science **gua** science. I have referred to this matter of a scientist's obligation to science in a lecture on the pursuit of science that I gave in threej India three ago. When I say obligations, I do not mean obligations to one's students, one's colleagues, or one's community. But obligations to science as science. It is not an easy concept.

For example, you cannot be naive enough to say "I must pay my indebtedness to the Principia; therefore I must do something like the Principia." It is absurd if not ridiculous for any normal person to suppose that he can, undertake writing something like the Principia. Nevertheless, what **is** one's obligation? Already in the forties I thought that Struve's approach to the problem was intolerant and egocentric. I am not criticizing him: I am only criticizing what he said when he prescribed the duty of others.

Krisciunas:

I'm glad to hear you say that. It gives me some justification when I question the way people in a higher position than I am are saying the way things ought to be done, and I've thought, well, you know, maybe they're not right.

Chandrasekhar:

People carry it very far.

Krisciunas:

Yes, they do.

Chandrasekhar:

Holton^[5] and others who write about Einstein's attitude to physics as something to emulate: I would say that Einstein's attitude to physics is probably the **worst** example anybody can follow, because nobody can be an Einstein. What was good for Einstein is most probably not good for anybody else.

Krisciunas:

Have you heard of this book? Arp just published it. This showed up in the mail the other day.

Krisciunas:

Yes, I know, that's why I've been trying to finish it before I came to talk to you. He presents a lot of very interesting evidence.

Chandrasekhar:

Does he say about my refusal...

Krisciunas:

. . yes, he hasn't mentioned you yet, but he says that, for example, "I wrote a paper with these people. Here is the evidence. And the **ASTROPHYSICAL JOURNAL** held it up for publication for a year and three months." Given time-scales today, that's not really holding it up. It just takes a while to come out. But there's some interesting pictures in...

Chandrasekhar:

I did publish as a supplement an atlas of Arp's. On the other hand, in my judgement he was quite unable to distinguish between a theoretical idea and a private intuitive notion. Whenever he sent me a paper I used to delete all his 'theoretical interpretations' and write to him that only the

observational parts of his paper could be published and that there was no place in the **Astrophysical Journal** for his 'theory.'

Krisciunas:

OK, let's get back a little bit to Struve. If he was running off to Texas often, and missing a lot of his lectures, it sounds like he wasn't at Yerkes often, or would have dinner parties or social events very often.

Chandrasekhar:

Struve rarely participated in social functions and social gatherings, particularly after the war. But even after the war, he used to take me out to lunch not infrequently. So far as I know, I was the only member of the faculty to whom he accorded that courtesy.

Krisciunas:

You would go to his house for lunch?

Chandrasekhar:

No! He used to take me out to lunch. Before the war, he did on occasion ask my wife and myself to his

home for a Sundai tea. After the war Mrs. Struve became very unsociable—I think it is not unfair to say that she became a recluse.

Krisciunas:

When did they get married?^[6]

Chandrasekhar:

I believe they must have been married in the early thirties. No, I'm sorry, they should have been married in the twenties, because I believe she was with him in Europe when he was on a Guggenheim at Cambridge in the late twenties — 1928?

Krisciunas:

So she was born in Europe.

Chandrasekhar:

No, I am fairly certain Mary Struve hailed from Michigan. I am equally certain that she was married before her marriage to Struve and had been divorced. Struve was her second husband.

Krisciunas:

OK. Who were Struve's friends? People outside of astronomy? Anybody you remember?

Chandrasekhar:

I know that Bobrovkikoff was one of his friends. He used to like Bobrovnikoff. And...

Krisciunas:

He's still alive, you know. He lives in a retirement home in Berkeley. He's about 92 now.

Chandrasekhar:

Bobrovnikoff may be able to throw some fresh light on Struve during the early years. But I do not know how good his memory is.

Krisciunas:

He's pretty feeble, from what Osterbrock says.

Chandrasekhar:

A person whom Struve admired most was Henry Norris Russell. He also admired Eddington and Milne. (Struve told me that Milne was awarded

Bruce Medal on his nomination.) I believe that Struve considered Cecilia Payne-Gaposchkin as a friend. Cecilia's biography^[7] (which I just read the other day), is very well written and self-effacing. She mentions that Struve was one of her best friends.

Krisciunas:

Oh, really?

Chandrasekhar:

Indeed; Struve was one of the few of his generation who appreciated Cecilia's early work on stellar atmospheres and stellar spectroscopy. Cecilia mentions this fact in her autobiography. Struve comes out very well in her account. In Cecilia's book there is a picture which is wrongly attributed to her daughter: she mis-identifies it as the one taken at the 1939 Conference on White Dwarfs and Novae in Paris. I wrote to Mrs. Haramundanis pointing out her error and sending her a copy of the original photograph. In this photograph Cecilia is standing on one side of Russell with Serge Gaposhkin on the other, in the front row. The photograph also includes Eddington and Walter Baade. In Katharine's book,

besides Struve, Baade is included as one of Cecilia's best friends.

I recall Struve telling me at the time, when he was the Chairman of the Astronomy Section of the National Academy of Sciences, that Cecilia was one of the candidates up for election; that she was only one vote short in the preliminary ballot; and that he tried his best to get somebody to change his vote (at that time there were no women members of the NAS!); but that he was unable to persuade anyone to do so. It is to the everlasting shame of the astronomers of that period that they never elected Cecilia to the NAS. In any event, Struve was not one of the chauvinists of the thirties; he was generous to Cecilia Payne: indeed, his sensitiveness to Cecilia's contributions was one of a pattern: he was **always** sensitive to those whom he considered to be devoted to astronomy. That was probably Struve's best characteristic. He never allowed personal differences, as for example with Kuiper, to interfere with his scientific judgement. In fact it was Struve who nominated Kuiper for membership in the National Academy.

Krisciunas:

You might say Struve was responsible for making the University of Chicago astronomy faculty into a real powerhouse, a real brain trust.

Chandrasekhar:

Yes, Struve certainly created a department at the University when there was none. But Struve, unlike others, was not an "empire builder." As I said, Struve was sensitive to those who did research with integrity and motivation; and he had those qualities abundantly himself. But Struve was not always right in his judgement, I can give examples in which he was wrong — but not in cases that were clear cut. It is not difficult for anyone to appreciate that Kuiper, during the thirties and forties, was one of the most discerning of astronomers. Struve accepted that fact in spite of his personal disagreements and personality conflicts with him.

Krisciunas:

Isn't it true, though, that by the late forties, Struve was kind of uncomfortable that he was no longer THE big fish in the pond, so to speak — that you

and Kuiper and Stromgren and Morgan now were well known in their own regard, and even without Struve, you all would continue to do recognizable things?

Chandrasekhar:

Others have said that; but I am not sure that is the right interpretation. Struve's great heroes of the past were Hale and William Huggins. Struve explicitly modelled his life on Hale. As you know, Hale ceased to be an active astronomer after 1916. For example, when he built the 100-inch telescope, he principally had Hubble in mind. And Struve, in the same way, felt that he could cease being an administrator but still receive the same loyalty and devotion from his colleagues in the way that Hale had from his. And that, I am afraid, was not accorded to him.

Krisciunas:

And Kuiper certainly wasn't the type of—apparently he got an ulcer trying to be the Director of Yerkes while Struve was still there, because he didn't really have free rein as director.

Chandrasekhar:

Well, part of that may be attributed to Kuiper's personality and temperament. During 1950, when I was the acting chairman of the department while Struve was still at Yerkes, I had no conflicts with him. I want to make one thing clear: In my personal judgement, Struve was not jealous of the reputations of his colleagues; but he felt that he was owed deference from them of an old-fashioned kind. That feeling was not reciprocated by most of his colleagues; and that is what contributed to his frustrated state of mind.

Krisciunas:

How much did you see him after he left Yerkes?

Chandrasekhar:

I did correspond with Struve for about a year, and less frequently in later years. But my correspondence with him, through the Journal office, continued through the rest of his life.

In spite of the fact that Struve and I were scientists in different molds, we always maintained a warm personal regard for each other. In many ways it was

characteristic of him that one of the first things he did when he became President of the Astronomical Society of the Pacific was to give me the Bruce Medal. When I visited him at Berkeley in March, 1953, on the occasion of the Bruce award, he accorded me both friendship and cordiality.

Krisciunas:

So that's counter to the notion that he was irritated in 1946 or 47, given that you became a Distinguished Service Professor shortly after he did. Was he irritated with that or not^[8]?

Chandrasekhar:

Let me put it this way.

Krisciunas:

I phrased this question badly.

Chandrasekhar:

The question already prejudices the issue. Was he irritated or not? My answer either way would be prejudicial.

Krisciunas:

Let me rephrase the question.

Chandrasekhar:

Let me state the facts. You may not know how Struve became a distinguished service professor at Chicago. The way his appointment came about was the following: After the war Hutchins had invited a great number of distinguished men to the University as members of the faculty. Marshall Stone, as a distinguished service professor in mathematics (without any consultations with the members of the mathematics department); Enrico Fermi as a distinguished service professor in physics; Harold Urey as a distinguished service professor in chemistry; and Gustav Rosby as a distinguished service professor in meteorology. But Hutchins overlooked Struve; and I sensed that Struve felt disappointed.

Discerning this disappointment, I asked for an appointment with Hutchins; and I told him, "You have appointed all these people as distinguished service professors. All the appointments you have made are well merited. But why haven't you thought

of appointing Struve to a distinguished service professorship?"

Krisciunas:

That's very interesting.

Chandrasekhar:

And Hutchins said to me, "I can kick myself for not thinking of it. I'm ashamed that I never thought of it. Thank you, Chandra, for telling me." That was the end of the meeting. And Struve had been made a distinguished service professor before a month had elapsed.

Krisciunas:

OK, so that's an independent set of events to — I believe you were, after Russell retired — you were offered a job there, and so they wanted to keep you here, and that's what happened, and so it was just the way events unfolded, OK? So it's entirely within Struve's relationship to you to give you the Bruce Medal because you're a friend and colleague and he thought you deserved it.

Chandrasekhar:

And I don't think Struve was upset in any way with my becoming a distinguished service professor. But he thought that Kuiper and Morgan would be upset.

Krisciunas:

Oh. OK.

Chandrasekhar:

I do not know to what extent Struve's appreciation of my scientific efforts was based on a real understanding of my work. In his book on astronomy with Vaéta Zebergs, he confuses what has sometimes been called the ChandrasekharSchoenberg limit with my limit for white dwarfs. The fact is that Struve always had an admiration (perhaps even an inferiority complex) with respect to the British astronomers: Eddington, Jeans, R.H. Fowler, and E.A. Mime. And he considered me as a part of the British tradition.

Also, I believe that my election to the Royal Society in 1944 meant more to Struve than what it would normally have meant to any American scientist. And so the fact that Hutchins made me a distinguished

service professor did not bother Struve a bit. In fact, he was responsible for my nomination to the Russell lectureship — the third after Russell and Adams.

Also, and this is a fact (I am sure not known) that he told me that he had nominated me as his successor to be the president of the American Astronomical Society; and that the Council had vetoed his suggestion. Perhaps I should not mention all that, but I am concerned with the prevalent misunderstanding that Struve and I did not continue with the warmest personal relations during the later years at Yerkes or after his departure to Berkeley and elsewhere.

Krisciunas:

So you didn't correspond with him that much after he left Yerkes?

Chandrasekhar:

No. As I have already said, I did correspond with him during the first year or two after he joined the Berkeley department; and after that through the Journal office. I do remember that when the Astrophysical Journal had expanded to 1,000 pages

per volume — its maximum during Struve's and Morgan's editorships had reached 600 pages per volume — Struve wrote a warm letter congratulating me on the way I was editing the Journal.

At a later time, I wrote to Struve asking him if he would not write to me some of his reminiscences concerning the Astrophysical Journal during the period of his editorship and send them to me. I thought that his recollections and reminiscences would be useful for the historical record. His response was pathetic, perhaps illustrating the state of his mind at that time. Struve wrote how he was in fact the editor of the Journal from 1932 onwards; and that even though his name appeared on the masthead beginning only in 1937, he really had been the editor since 1932; and that he had kept Frost's name on the masthead solely for the purpose of not offending him.

Moreover, he thought that it was unfair that when the general index for Volumes 76 to 100 of the Journal was published (soon after I became the managing editor), in the list of managing editors, I had recorded Struve's editorship as starting with volume 82. And so when I wrote the brief obituary

notice for Struve (Ap. J. 139 (1964), p. 423), I had the caption under his photograph read 'Managing Editor, Astrophysical Journal 1932-1947.' After this note had been published, I had a letter from Mary Struve saying that "It was the best that could have been written about Otto", and that he would have appreciated what I wrote.

Krisciunas:

In my book^[9] I quote a friend of mine as saying that the appropriately put-together astronomical library, if you don't have a lot of money, contains two copies of the Ap. J. and no other technical journals whatsoever. So you might want **SKY AND TELESCOPE**.

Struve died in 1963, and from what I've read so far, in his last couple of years he seemed to be kind of lost. He went to Greenbank and apparently didn't — some people say he didn't — do a very good job there, that he was the wrong man for the job at the time.

Chandrasekhar:

I've heard it said that Struve's directorship at Greenbank was a failure. You must know that when he returned to Berkeley he did not want to mix with other people, would not move to the new "Campbell Building" (?), and continued to stay in that small hut.

Krisciunas:

How is it that you know, after he died^[10], that his gold medals were melted down and his astronomical effects were somewhat dispersed? Is this people mentioning, "Oh, you must know," or...?

Chandrasekhar:

The way I learned about the dispersal of Struve's astronomical and related material was the following: When I learned of Struve's death, I phoned someone at Berkeley: perhaps it was Louis Henyey — asking him when a memorial service would be held for Struve. I naturally assumed that a memorial service **would** be held, and I had called to volunteer myself as a speaker at the memorial, representing the University of Chicago at the service. I was astonished to learn that Mrs. Struve had vetoed the

idea of a memorial service. Some years later, when I was in Berkeley on some other occasion (it must have been in the late sixties), someone told me - - perhaps it was Louis Henyey again — that all of Struve's effects had been dispersed and further, that his gold medals had been melted down.

Some years later, Naomi Greenstein told us (when my wife and I visited the Greensteins in Pasadena) that at one time when she was in Berkeley, she had tried to call on Mrs. Struve; but that no one would answer the bell. She therefore left a note to say that she would like to have Mrs. Struve call her (at the hotel?); and Mrs. Struve **did** call her; and that they talked on the telephone for a very long time. So as far as I know Naomi Greenstein was probably the last person who had talked extensively to Mary Struve.

Krisciunas:

She's no longer alive, is she?

Chandrasekhar:

Oh no, she died. She is supposed to have committed suicide. You didn't know that?

Krisciunas:

No, I didn't know that. When was that?

Chandrasekhar:

I think you should ask Naomi Greenstein. It was certainly in the late sixties. What I do know is that the neighbors found that the doors to Mrs. Struve's house — she was living there by herself after Otto's death — were locked and no one could see anyone leaving or entering the house. Apparently the neighbors called the police, who broke into the house and found that Mrs. Struve had died some days earlier.

Krisciunas:

Wow. Was that recent, soon after Otto Struve died?

Chandrasekhar:

After two or three years.

Krisciunas:

Two or three years?

Chandrasekhar:

It might have been four or five years, because Naomi Greenstein did talk to Mary Struve after Struve's death. I'm sure, I know that — in fact, she told me that.

Krisciunas:

Was Otto Struve a religious person at all? Did he ever quote the Bible or...?

Chandrasekhar:

No.

Krisciunas:

Or go to church on Easter or...?

Chandrasekhar:

Of course, he knew I was an atheist, and he never brought up the subject with me.

Krisciunas:

Was he buried in California, do you know?

Chandrasekhar:

I believe he was cremated.

Krisciunas:

Well, let's turn to something a bit cheerier than that. What do you see as Struve's legacy? To what extent did he pay a debt to science? What did he leave us as a good example?

Chandrasekhar:

The "legacy" which any normal individual leaves is mostly anonymous. A scientific legacy as conventionally defined and attributed to a person, exists only with respect to the greatest names: Newton...

Krisciunas:

. . . Saha, Einstein maybe. (Pictures on Chandra's wall)

Chandrasekhar:

Well, certainly Einstein. Perhaps in this century only a half a dozen names — Einstein, Bohr, Heisenberg, Dirac, Fermi, and a few others of that rank—will remain. The contributions of others of less stature will get integrated into the commonplaces of science—and this sometimes happens even with the

greatest of discoveries. Take, for example, the nuclear model of the atom. During the period 1910-1930 it used to be referred to, correctly, as the Rutherford-Bohr model. But no one uses that description today: it has become a commonplace of science.

Krisciunas:

The Bohr theory of the atom, they still call it that.

Charidrasekhar: But it is the Bohr frequency condition, that has survived. And the Rutherford law of scattering has also survived. But ...

Krisciunas:

Fermi-Dirac statistics, I think some names will stick for a while. Chandrasekhar's limit. Thatts even in a couple of songs I know.

Chandrasekhar:

I think that the survival of names which get attached to some concept or phenomena is in some sense unfair. In the Notes and Records of the Royal Society for 1973 — Freeman Dyson quotes from

this article in a public lecture of his—I recall a conversation in which Eddington, Rutherford, and two or three others participated during the Christmas holidays of 1933 at Trinity College, Cambridge. One of the participants was Sir Maurice Amos, a retired chief justice of the Egyptian court. Amos asked Rutherford, "I do not see why Einstein is accorded a greater public acclaim than you. After all, you invented the nuclear model of the atom; and that model provides the basis for all physical science today and it is even more universal in its applications than Newton's laws of gravitation. Whereas Einstein's predictions refer to such minute departures from the Newtonian theory that I do not see what all the fuss is about." Rutherford, in response, turned to Eddington and said, "You are responsible for Einstein's fame." Why is Einstein more famous than Rutherford? Who can say?

You asked me about Struve's legacy. Everybody talks about HI and HII regions: they call them the Strömberg spheres; but the discovery that stimulated Strömberg was Struve's. Only very few astronomers know that. And that is the way contributions to science become anonymous. As another example, take the matter of the abundance of hydrogen. The

person who completely resolved the problem was Rupert Wildt. Hardly anyone knows his name now.

Krisciunas:

I know his name. He died in 1976.

Chandrasekhar:

I was recently at a meeting in Washington concerned with the history of some recent advances in Astrophysics. There was a discussion about the abundance of hydrogen. The name of Henry Norris Russell was mentioned; but not of Cecilia Gaposchkin, who preceded Russell, nor of Rupert Wildt, who followed with his complete resolution of the problem. I have talked to some of my colleagues at Chicago who presently give courses on stellar atmospheres. And I have on occasion questioned them about the negative hydrogen ion and Wildt's role in the recognition of the high abundance of hydrogen. The response has almost always been a blank.

I mention these facts not as a criticism but as how discoveries in science become anonymous. And Struve has become anonymous.

Krisciunas:

Well, he was very prolific. He ranks right up there near the top, as far as how much work he produced is concerned.

Chandrasekhar:

The telescope at McDonald is now called the Struve telescope. But what Struve did for astronomy is ever so much greater; and he's now to be known through a telescope having been named after him. That illustrates the unfairness in the way names get attached to things.

A personal note: you mentioned the white dwarf limit. I am often asked "Aren't you pleased?" My response often is, "Should I be pleased, when something I did as a young man of twenty is selected?" Am I to assume that my scientific efforts of the following fifty-five years have been in vain? I simply do not believe that having a name perpetuated by having something called by that name is a measure of that person's contributions."

Struve's contributions to astrophysics and astronomy are many and varied: the recognition of the Stark

effect in stellar atmospheres; the role of turbulence; stellar rotation; the exchange of matter among close binaries; the discovery of HI-HII regions; all of them bearing on our present understanding of astronomy. Struve's work, particularly on the exchange of matter in close binaries, plays an important role in our current ideas relating to stellar evolution, particularly the formation of pulsars; and in many other regions. The influence of Struve's work has not disappeared. It is not forgotten. It has simply become a part of everyone's knowledge. I would only say that the word 'legacy' is not the right word to describe the effect of a person's contribution to science.

Krisciunas:

From what I've gathered, though, talking to other people who knew him or reading things about him, his legacy seems to be as a fellow who worked really hard, in fact perhaps too hard, so that he got angry now and then, and other people felt maybe they had to—we've already talked about some of that. I'm not making very good sense. One last question I thought of about Struve. Somebody told me once that the way he used to write so much is that he used to bring

somebody to take dictation^[11] while he was observing, you know, with the stars trailing across the slit, so he'd have five minutes till it got to the other end and he had to flip a little lever, so he'd dictate to somebody so that he could keep writing faster than just observing.

Chandrasekhar:

That story must have originated at McDonald. I cannot confirm it since the only occasion I was at McDonald was at its dedication in 1939. However, during the five years I was an associate editor of the *Astrophysical Journal* during the time Struve was managing editor, I saw many manuscripts of his written in long hand and subsequently typed by his secretary, Alice Johnson. Also, during the war years when he was collaborating with Swings, I have seen drafts of many of their joint papers in Struve's handwriting. I also know that while he was measuring plates, he used to have one of his secretaries record the measurements while he was doing them.

On the other hand, with regard to his having dictated while at the telescope, it could be an exaggeration.

Let me give you an example from my own personal experience. When I was a student in India, in my final college years I used to bicycle every morning to attend a German class between 7 and 8 o'clock; and while I was riding, I used to have a German grammar—book on hand, glancing at it periodically, to memorize the various declinations. That was during 1928-1929. Some ten years later, when I returned to India, the story was being told that as a student I was so absorbed in science that I used to read scientific books while bicycling to college -a total exaggeration with some element of truth. The story about Struve could be an exaggeration of that kind.

Krisciunas:

Here's another question. When you have a seminar, I think you can tell a lot about a person by what kind of questions they ask the speaker. What was he like listening to an— other speaker?

Chandrasekhar:

He very rarely asked questions.

Krisciunas:

He very rarely asked questions?

Chandrasekhar:

He did ask questions if some matter explicitly relating to his own work was under consideration. I remember one occasion when Anne Underhill gave a colloquium on Stark effect in stellar spectra soon after her arrival at Yerkes as a graduate student. She was a Canadian; had been a student of Helen Hogg (or Vibert Douglas?) at Toronto. And I believe that she had written some papers critical of some of Struve's findings. At the colloquium, she repeated her criticisms. At the end, Struve was merciless in the way he reacted to her criticisms. Anne Underhill was practically in tears.

On another occasion, after the war I gave a colloquium on stellar associations based upon some papers by Ambartsumian that he had sent me through a friend in England. (I believe the occasion was when Ambartsumian was in England in connection with the meeting of the 1946 Royal Society Newton Tercentenary Celebrations. My friend, Professor K.S. Krishnan, was also at that

meeting; and Ambartsumian sent his papers through Krishnan.) I am fairly certain that my colloquium on stellar associations was the first that had been given anywhere on them, since Ambartsumian had written his papers during the war and they were not available in the west. In any event, I started the colloquium with some remarks relating to focal lengths of telescopes, their resolving power, etc. I must have made some very obvious and elementary mistakes in that context. Struve and Kuiper had no difficulty in demolishing what I had said. Anyhow, the test of my talk on Ambartsumian's ideas on stellar associations was treated equally critically — not only by Struve, but also by Kuiper and Morgan. Ironically, all of them who criticized me were later to be great promoters of Ambartsumian's ideas.

Krisciunas:

How much time have we got?

Chandrasekhar:

Well, I have arranged to go to lunch today at the Club at 12 o'clock.

Krisciunas:

OK. Let me put a different tape in here, and I'd like to talk about you for a while, OK?

Chandrasekhar:

I do not know if there is very much to say about myself since apparently you have read the transcript of the tape that is at the ALP.

Krisciunas:

Oh, but it's ten years old.

Chandrasekhar:

Nothing much has happened since.

Krisciunas:

Oh, you wrote a very long book, three copies of which are sitting at the bookstore here. And you've won a Nobel Prize. Has that been an inconvenience? Dick Feynman says it's complicated his life tremendously, because people only want to know who he is because he has that. But we all knew who you were before you won that, and when you did, we all thought, well, it's about the time that astronomers

were getting some—I would consider you in with the astronomers that are getting more recognition than they were.

Chandrasekhar:

I know that many people have written about how having received the Nobel prize has affected them. I can only say that: It hasn't made any difference to me. It has not affected me: I never expected it.

Krisciunas:

You must have done something with the prize money.

Chandrasekhar:

I really do not know if 95,000 dollars is that much money. The annual interest that one can obtain from it adds to the amount one receives from TIAA and CREF^[12]. It doesn't make that much difference.

Krisciunas:

I have that too.

Chandrasekhar:

I don't see that money-wise it is —

Krisciunas:

But it didn't change your life in any major way? My father works at Argonne National Lab, and he said that for a while they were trying to get you to give a talk out there.

Chandrasekhar:

I didn't.

Krisciunas:

And he said you were too busy.

Chandrasekhar:

No, as a rule I decline most invitations for colloquia and seminars unless I feel that a special circumstance requires a positive response. Since you have persisted in asking me how the Nobel prize has affected me, let me read my response at an occasion arranged by the Museum of Science and Industry. I thought a good deal as to what I should say on that occasion. They have, in the Museum of Science and

Industry, a "Nobel Hall," in which they have paintings of all Americans who have received the Nobel prize. While an occasion such as that one at the Museum of Science and Industry, may be personally very gratifying.

"I must confess to some misgivings as to the appropriateness of selecting for special honor those who have received recognition of a particular kind by one's contemporaries. I am perhaps overly sensitive to this issue, since I've always remembered what a close friend of earlier years, Professor Edward Arthur Milne, once reminded me; and parenthetically may I note that Arthur Milne is one of the great pioneers of modern theoretical astrophysics. On an occasion now more than fifty years ago, Milne reminded me that posterity in time will give us all our true measure, and assign to each of us our due and humble places, and that in the end, it is the judgement of posterity that really matters. And he further added, 'He really succeeds who perseveres according to his likes unaffected by fortune, good or bad. It is well to remember that there is in general no correlation between the judgement of posterity and the judgement of contemporaries.' I hope you will forgive me if I

allow myself a personal reflection. During the seventies, I experienced two major heart episodes. Suppose one of them had proved fatal, as it well might have? Then there would have been no cause for celebration. But I hope that the judgement of posterity, of my efforts in science, would not have been diminished on that account. Conversely, I hope that it would not be enhanced on account of a doctor's skills."

Now, that about states my views. I think whether it affects your life or not depends upon your own personal attitude. I never thought that the recognition that one receives in one's lifetime is a measure of what one is. So I don't think...

Krisciunas:

In a couple of your articles, you mentioned at the very end that you're fond of an ancient maxim, that "The simple is the seal of the true." To what extent do you consider some of the things you've been working on as true, and/or simple?

Chandrasekhar:

That statement is a description of scientific truths. But that does not mean that it necessarily applies to my work. The quotation you attribute to me is in the Ryerson lecture I gave here at the University in 1975. I believe that that statement was made in the context of Kerr's discovery of his solution. I say that my recognition of the significance of the discovery by Kerr of his solution was the most shattering experience of my life; and, the fact that the search for the beautiful in the abstract should have an exact replica in nature is an example of how "the simple is the seal of the true." It is a description of the fundamental truths of science.

Now, having said that, let me say that one can look at beauty in science in a whole variety of ways. If you ask a great painter, or a great sculptor, he will probably say he strives for the beautiful. But what about those who are not Michaelangelos or Raphaels? Can't they strive for beauty? The point here is, every painter, every sculptor tries to have some creation of his which he can call his own, which gives him a certain pleasure and that pleasure

need not be one which will be assessed in the way in which it appeals to him. It is one's own perception.

Or to put it another way, you may want to climb a mountain. You don't climb the Everest; your sights are not so ambitious. But when you do reach the top of the mountain, you see the valley below; and that gives you a sense of contentment. Well, one can perceive science that way. My own effort in science is to create a pattern for myself which appeals to me. And I can go further and say that to achieve a pattern is much more difficult than to write a paper.

In my book on black holes to which you have referred, in the epilogue, I refer to beauty in science. Now, it is a very much more difficult matter to ask yourself the following question. Let us take a concrete example. General relativity is a beautiful theory. How do you utilize your sensitiveness to that beauty to create something which will enhance your understanding of that theory? In order to understand general relativity, you have to know differential geometry. Knowing differential geometry, you can cultivate general relativity. How does your knowledge that the theory is beautiful enable you to cultivate it? That's a very much more difficult

question. And I've tried to answer that in a lecture I gave in Hamburg, Germany last year, in honor of Karl Schwarzschild. I'll give you a reprint of that.

It all comes down to your motivation. You ask me, what is one's legacy in science? A more relevant question one might ask is: what did the cultivation of science mean to that person? Does one measure it by the honors one receives? Is it meaningful because one has made an important discovery? What is it that gives substance and meaning to a life devoted to scientific endeavor? It is not easy to answer these questions.

I always give one example, as to the nature of scientific sensibility. A description I can give of my feelings is this: I do not mean it as a general rule. I remember that when I was a student in Cambridge, I spent a vacation in the moors in Perthshire in Scotland: I had a bicycle on which I used to go around the whole region, and I remember once going up the top of a hill—it was raining—and as I came down from the top of the hill, I suddenly saw the valley below in sunshine, and there was a perfectly circular rainbow — and a double rainbow at that. You can ask, was that a discovery of mine?

Rainbows have been known for centuries. But to me the recollection of that impression is still meaningful. What is one's legacy to science? How is one to know? It becomes anonymous. And few of us, very few of us, probably none of us, can hope to have a part in the future of science the way people like Newton and Galileo have had. Then is it worthless to do science? What is it one wants to gain?

Krisciunas:

I can't help it. You can't help it either, right?

Chandrasekhar:

Well, I am not sure. Does a painter have to paint?

Krisciunas:

Yes. Most of them do.

Chandrasekhar:

But what is it they are trying to paint? What is it they are trying to achieve? It must be more than painting one's house.

Krisciunas:

Trying to give their lives some meaning by creating something. I think.

Chandrasekhar:

I don't want to say that you just...

Krisciunas:

. . otherwise life is only material objects.

Chandrasekhar:

That is making a value out of it.

Krisciunas:

Well, that's my own perspective.

Chandrasekhar:

You can ask of the value that you have derived from your efforts: what is the value to me that I witnessed a rainbow?

Krisciunas:

You still think it's beautiful, years later.

Chandrasekhar:

Yes. So that, you see, it is not in terms of an absolute value. It is in terms of purely personal values.

Krisciunas:

OK.

Chandrasekhar:

And there is no external reference for the value of that experience. There is no act of creation involved in that. You witness something. I don't see why your value judgement should be involved in a motivation. I don't see why.

Krisciunas:

Well, we all evolve preferences for things we like to do and things we don't want to do. Riding a bike outside, you get some sunshine, you see a rainbow; you have to be in the right place to see it. It's better than getting rained on and getting cold, for example.

Chandrasekhar:

Well, I agree with the content of your English sentence. But that doesn't provide the right nuance for me.

Krisciunas:

Let me ask you a different question. You mentioned earlier that you think a lot of astronomers go about it the wrong way. Here's kind of an open-ended question. Do you think that astronomy is more of a qualitative or a quantitative activity? Where else can you see an equation that has "approximately greater than"? That's pretty mind boggling to people who aren't astronomers, and you see that in the **Ap.J.** all the time.

Chandrasekhar:

Well, to take an example, which is probably not at all a fair one: in the 19th century, people went to Africa or Australia and discovered many kinds of new animals. They obviously added to our knowledge. Astronomy can be pursued in that way. It appeals to one's wonderment. But is science just wonderment? I was in VLA^[13] the other day, and

they showed me some incredible things in nature. Well, we understand none of them: at least so I was told. They conclude that they must make more observations. That is almost always how an astronomer concludes after his findings.

Krisciunas:

We need bigger telescopes.

Chandrasekhar:

So we can see more?

Krisciunas:

I've got them here somewhere. One of the things that motivates me is to try to demonstrate that you don't need onehundred-million dollars to do some new science. Here is some infra-red array images of Comet Halley, OK, and it was very faint then, just a little blob that's moving up to the left, OK. Here's a picture when it was pretty close to earth. It's the same image processing system. This was done on a six-inch telescope.

Chandrasekhar:

Icko Iben once told me that whenever he got a new result he wanted a bigger computer to go further. I told him that it was fortunate that the constraints were of an external kind, not constraints derived from his personal limitations.

[On the way to lunch Chandrasekhar mentioned that he thought the two greatest astronomers of the twentieth century were Karl Schwarzschild and Jan Oort.]

